Autobiography

SIGMUND FREUD

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By SIGMUND FREUD

Perhaps no one man has exerted so powerful an influence on contemporary thought as has Sigmund Freud. His theories have become part of the texture of our times and have left their impress on almost every field of ideas, popular as well as scientific. Beginning with The Interpretation of Dreams, a series of books appeared elaborating Freud's discoveries, which aroused ever more violent storms of criticism and praise. Gradually controversy has given ground before the growing recognition of Freud's contributions to an understanding of human nature.

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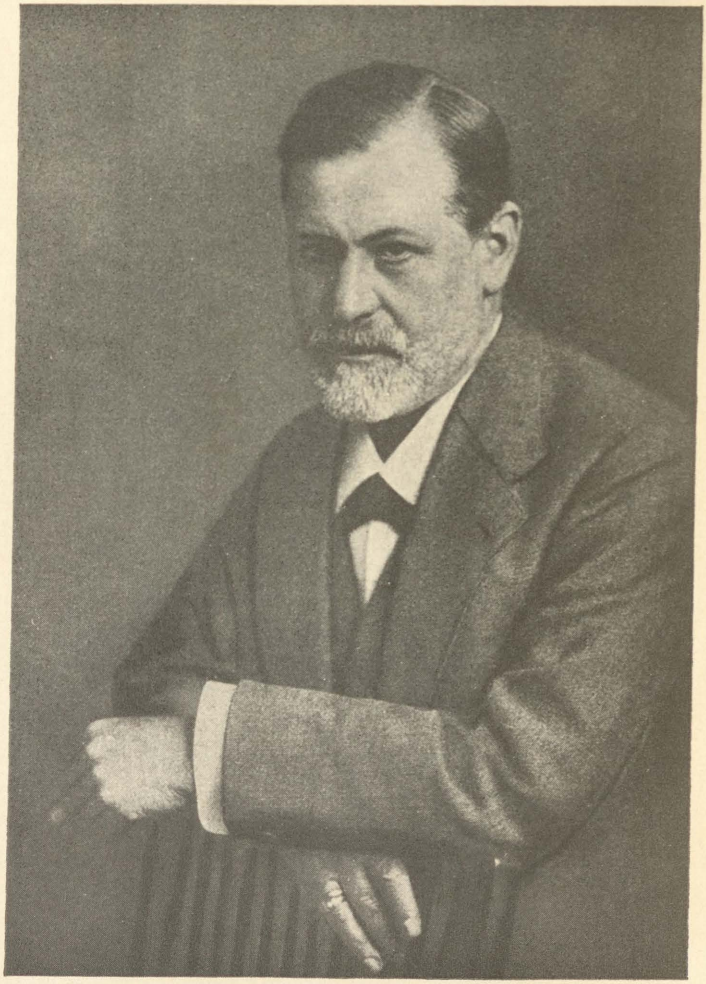

Sigm. Freud

(1909)

Autobiography

SIGMUND FREUD

Translated by JAMES STRACHEY

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AUTOBIOGRAPHY

I

SEVERAL of the contributors to this series of "Autobiographical Studies" [1] have begun by expressing their misgivings at the unusual difficulties of the task they have undertaken. The difficulties in my case are, I think, even greater; for I have already more than once published papers upon the same lines as the present one, papers which, from the nature of the subject, have dealt more with personal considerations than is usual or than would otherwise have been necessary.

I gave my first account of the development and subject-matter of psycho-analysis in five lectures which I delivered in 1909 before Clark University at Worcester, Mass., where I had been

[1] The present work appeared originally in Volume 4 of *Die Medizin der Gegenwart in Selbstdarstellungen* (Leipzig: Felix Meiner, 1925), a collection of autobiographical studies by leaders of the medical profession.

invited to attend the celebration of the twentieth anniversary of the foundation of that body.[2] Only recently I gave way to the temptation of making a contribution of a similar kind to an American collective publication dealing with the opening years of the twentieth century, since its editors had shown their recognition of the importance of psycho-analysis by allotting a special chapter to it.[3] Between these two dates appeared a paper, "On the History of the Psycho-Analytic Movement," [4] which, in fact, contains the essence of all that I can say on the present occasion. Since I must not contradict myself and since I have no wish to repeat myself exactly, I must endeavor to construct a narrative in which subjective and

[2] The lectures were first published (in English) in the *American Journal of Psychology* (1910); the original German was issued under the title of *Ueber Psychoanalyse* (Vienna, 1910).

[3] *These Eventful Years* (New York, 1924). My essay, translated by Dr. A. A. Brill, forms Chapter LXXIII of the second volume. [The original German appears in Vol. XI of Freud's *Gesammelte Schriften* (Vienna, 1927).]

[4] Published in the *Jahrbuch der Psychoanalyse,* Vol. VI, 1914. [English translation in Freud's *Collected Papers,* Vol. I.]

objective attitudes, biographical and historical interests, are combined in a new proportion.

I was born on May 6th, 1856, at Freiberg in Moravia, a small town in what is now Czecho-Slovakia. My parents were Jews, and I have remained a Jew myself. I have reason to believe that my father's family were settled for a long time on the Rhine (at Cologne); that, as a result of a persecution of the Jews during the fourteenth or fifteenth century, they fled eastwards, and that, in the course of the nineteenth century, they migrated back from Lithuania through Galicia into German Austria. When I was a child of four I came to Vienna, and I went through the whole of my education there. At the "Gymnasium" I was at the top of my class for seven years; I enjoyed special privileges there, and was required to pass scarcely any examinations. Although we lived in very limited circumstances, my father insisted that, in my choice of a profession, I should follow my own inclina-

tions. Neither at that time, nor indeed in my later life, did I feel any particular predilection for the career of a physician. I was moved, rather, by a sort of curiosity, which was, however, directed more towards human concerns than towards natural objects; nor had I grasped the importance of observation as one of the best means of gratifying it. My early familiarity with the Bible story (at a time almost before I had learned the art of reading) had, as I recognized much later, an enduring effect upon the direction of my interest. Under the powerful influence of a school friendship with a boy rather my senior who grew up to be a well-known politician, I developed a wish to study law like him and to engage in social activities. At the same time, the theories of Darwin, which were then of topical interest, strongly attracted me, for they held out hopes of an extraordinary advance in our understanding of the world; and it was hearing Goethe's beautiful essay on Nature read aloud at a popular lecture by Professor Carl

Brühl just before I left school that decided me to become a medical student.

When, in 1873, I first joined the University, I experienced some appreciable disappointments. Above all, I found that I was expected to feel myself inferior and an alien because I was a Jew. I refused absolutely to do the first of these things. I have never been able to see why I should feel ashamed of my descent or, as people were beginning to say, of my race. I put up, without much regret, with my non-acceptance into the community; for it seemed to me that in spite of this exclusion an active fellow-worker could not fail to find some nook or cranny in the framework of humanity. These first impressions at the University, however, had one consequence which was afterwards to prove important; for at an early age I was made familiar with the fate of being in the opposition and of being put under the ban of the "compact majority." The foundations were thus laid for a certain degree of independence of judgment.

I was compelled, moreover, during my first years at the University, to make the discovery that the peculiarities and limitations of my gifts denied me all success in many of the departments of science into which my youthful eagerness had plunged me. Thus I learned the truth of Mephistopheles' warning:

Vergebens, dass ihr ringsum wissenschaftlich
schweift,
Ein jeder lernt nur, was er lernen kann.[5]

At length, in Ernst Brücke's physiological laboratory, I found rest and satisfaction—and men, too, whom I could respect and take as my models: the great Brücke himself, and his assistants, Sigmund Exner and Ernst von Fleischl-Marxow. With the last of these, a brilliant man, I was privileged to be upon terms of friendship. Brücke gave me a problem to work out in the

[5] ["It is in vain that you range around from science to science: each man learns only what he can learn." *Faust,* Part I.]

histology of the nervous system; I succeeded in solving it to his satisfaction and in carrying the work further on my own account. I worked at this institute, with short interruptions, from 1876 to 1882, and it was generally thought that I was marked out to fill the next post of Assistant that might fall vacant there. The various branches of medicine proper, apart from psychiatry, had no attraction for me. I was decidedly negligent in pursuing my medical studies, and it was not until 1881 that I took my somewhat belated degree as a Doctor of Medicine.

The turning point came in 1882, when my teacher, for whom I felt the highest possible esteem, corrected my father's generous improvidence by strongly advising me, in view of my bad financial position, to abandon my theoretical career. I followed his advice, left the physiological laboratory and entered the General Hospital [6] as an "Aspirant." I was soon afterwards promoted to being a junior resident physician, and worked

[6] [The principal hospital in Vienna.]

in various departments of the hospital, amongst others for more than six months under Meynert, by whose work and personality I had been greatly struck while I was still a student.

In a certain sense I nevertheless remained faithful to the line of work upon which I had originally started. The subject which Brücke had proposed for my investigations had been the spinal cord of one of the lowest of the fishes (*Ammocoetes Petromyzon*); and I now passed on to the human central nervous system. Just at this time Flechsig's discoveries of the non-simultaneity of the formation of the medullary sheathes were throwing a revealing light upon the intricate course of its tracts. The fact that I began by choosing the *medulla oblongata* as the one and only subject of my work was another sign of the continuity of my development. In complete contrast to the diffuse character of my studies during my earlier years at the University, I was now developing an inclination to concentrate my work exclusively upon a single subject

or problem. This inclination has persisted and has since led to my being accused of one-sidedness.

I now became as active a worker in the Institute of Cerebral Anatomy as I had previously been in the physiological one. Some short papers upon the course of the tracts and the nuclear origins in the *medulla oblongata* date from these hospital years, and my results were regularly noted down by Edinger. One day Meynert, who had given me access to the laboratory even during the times when I was not actually working under him, proposed that I should definitely devote myself to the anatomy of the brain, and promised to hand over his lecturing work to me, as he felt he was too old to manage the newer methods. This I declined, in alarm at the magnitude of the task; it is possible, too, that I had guessed already that this great man was by no means kindly disposed towards me.

From the practical point of view, brain anatomy was certainly no better than physiology,

and, with an eye to material considerations, I began to study nervous diseases. There were, at that time, few specialists in that branch of medicine in Vienna, the material for its study was distributed over a number of different departments of the hospital, there was no satisfactory opportunity of learning the subject, and one was forced to be one's own teacher. Even Nothnagel, who had been appointed a short time before, on account of his book upon cerebral localization, did not single out neuropathology from among the other sub-divisions of medicine. In the distance glimmered the great name of Charcot; so I formed a plan of first obtaining an appointment as Lecturer on Nervous Diseases in Vienna and of then going to Paris to continue my studies.

In the course of the following years, while I continued to work as a junior physician, I published a number of clinical observations upon organic diseases of the nervous system. I gradually became familiar with the ground; I was able

to localize the site of a lesion in the *medulla oblongata* so accurately that the pathological anatomist had no further information to add; I was the first person in Vienna to send a case for autopsy with a diagnosis of *polyneuritis acuta.*

The fame of my diagnoses and of their *post-mortem* confirmation brought me an influx of American physicians, to whom I lectured upon the patients in my department in a sort of pidgin-English. I understood nothing about the neuroses. On one occasion I introduced to my audience a neurotic suffering from a persistent headache as a case of chronic localized meningitis; they quite rightly rose in revolt against me, and my premature activities as a teacher came to an end. By way of excuse I may add that this happened at a time when greater authorities than myself in Vienna were in the habit of diagnosing neurasthenia as cerebral tumor.

In the spring of 1885 I was appointed Lecturer on Neuropathology on the ground of my histological and clinical publications. Soon after-

wards, as the result of a warm testimonial from Brücke, I was awarded a Travelling Fellowship of considerable value. In the autumn of the same year I made the journey to Paris.

I became a student at the Salpêtrière, but, as one of the crowd of foreign visitors, I had little attention paid me to begin with. One day in my hearing Charcot expressed his regret that since the war he had heard nothing from the German translator of his lectures; he went on to say that he would be glad if someone would undertake to translate the new volume of his lectures into German. I wrote to him and offered to do so; I can still remember a phrase in the letter, to the effect that I suffered only from *"l'aphasie motrice"* and not from *"l'aphasie sensorielle du français."* Charcot accepted the offer, I was admitted to the circle of his personal acquaintances, and from that time forward I took a full part in all that went on at the clinic.

As I write these lines, a number of papers and newspaper articles have reached me from France,

which give evidence of a violent objection to the acceptance of psycho-analysis, and which often make the most inaccurate assertions in regard to my relations with the French school. I read, for instance, that I made use of my visit to Paris to familiarize myself with the theories of Pierre Janet and then made off with my booty. I should therefore like to say explicitly that during the whole of my visit to the Salpêtrière Janet's name was never so much as mentioned.

What impressed me most of all while I was with Charcot were his latest investigations upon hysteria, some of which were carried out under my own eyes. He had proved, for instance, the genuineness of hysterical phenomena and their conformity to law (*"introite et hic dii sunt"*), the frequent occurrence of hysteria in men, the production of hysterical paralyses and contractures by hypnotic suggestion and the fact that such artificial products showed, down to their smallest details, the same features as spontaneous attacks, which were often brought on traumati-

cally. Many of Charcot's demonstrations began by provoking in me and in other visitors a sense of astonishment and an inclination to skepticism, which we tried to justify by an appeal to one of the theories of the day. He was always friendly and patient in dealing with such doubts, but he was also most decided; it was in one of these discussions that (speaking of theory) he remarked, "*Ça n'empêche pas d'exister,*" a *mot* which left an indelible mark upon my mind.

No doubt the whole of what Charcot taught us at that time does not hold good today: some of it has become doubtful, some has definitely failed to withstand the test of time. But enough is left over and has found a permanent place in the storehouse of science. Before leaving Paris I discussed with the great man a plan for a comparative study of hysterical and organic paralyses. I wished to establish the thesis that in hysteria paralyses and anaesthesias of the various parts of the body are demarcated according to the popular idea of their limits and not according to ana-

tomical facts. He agreed with this view, but it was easy to see that in reality he took no special interest in penetrating more deeply into the psychology of the neuroses. When all is said and done, it was from pathological anatomy that his work had started.

Before I returned to Vienna I stopped for a few weeks in Berlin, in order to gain a little knowledge of the general disorders of childhood. Kassowitz, who was at the head of a public institute in Vienna for the treatment of children's diseases, had promised to put me in charge of a department for the nervous diseases of children. In Berlin I was given assistance and a friendly reception by Baginsky. In the course of the next few years I published, from the Kassowitz Institute, several monographs of considerable size on unilateral and bilateral cerebral paralyses in children. And for that reason, at a later date (in 1897), Nothnagel made me responsible for dealing with the same subject in his great *Handbuch der allgemeinen und speziellen Therapie.*

In the autumn of 1886 I settled down in Vienna as a physician, and married the girl who had been waiting for me in a distant city for more than four years. I may here go back a little and explain how it was the fault of my fiancée that I was not already famous at that early age. A side interest, though it was a deep one, had led me in 1884 to obtain from Merck some of what was then the little-known alkaloid cocaine and to study its physiological action. While I was in the middle of this work, an opportunity arose for making a journey to visit my fiancée, from whom I had been parted for two years. I hastily wound up my investigation of cocaine and contented myself in my book on the subject with prophesying that further uses for it would soon be found. I suggested, however, to my friend Königstein, the ophthalmologist, that he should investigate the question of how far the anaesthetizing properties of cocaine were applicable in diseases of the eye. When I returned from my holiday I found that not he, but another of my

friends, Carl Koller (now in New York), whom I had also spoken to about cocaine, had made the decisive experiments upon animals' eyes and had demonstrated them at the Ophthalmological Congress at Heidelberg. Koller is therefore rightly regarded as the discoverer of local anaesthesia by cocaine, which has become so important in minor surgery; but I bore my fiancée no grudge for her interruption of my work.

I will now return to the year 1886, the time of my settling down in Vienna as a specialist in nervous diseases. The duty devolved upon me of giving a report before the "Gesellschaft der Aerzte" [Society of Medicine] upon what I had seen and learned with Charcot. But I met with a bad reception. Persons of authority, such as the Chairman (Bamberger, the physician), declared that what I said was incredible. Meynert urged me to find some cases in Vienna similar to those which I had described and to present them before the Society. I tried to do so; but the senior physicians in whose departments I found

any such cases refused to allow me to observe them or to work at them. One of them, an old surgeon, actually broke out with the exclamation: "But, my dear sir, how can you talk such nonsense? *Hysteron* [sic] means the uterus. So how can a man be hysterical?" I objected in vain that what I wanted was not to have my diagnosis approved, but to have the case put at my disposal. At length, outside the hospital, I came upon a case of classical hysterical hemi-anaesthesia in a man, and demonstrated it before the "Gesellschaft der Aerzte." This time I was applauded, but no further interest was taken in me. The impression that the high authorities had rejected my innovations remained unshaken; and, with my hysteria in men and my production of hysterical paralyses by suggestion, I found myself forced into the Opposition. As I was soon afterwards excluded from the laboratory of cerebral anatomy and for a whole session had nowhere to deliver my lectures, I withdrew from academic life and ceased to attend the

learned societies. It is a whole generation since I have visited the "Gesellschaft der Aerzte."

Anyone who wanted to make a living from the treatment of nervous patients must clearly be able to do something to help them. My therapeutic arsenal contained only two weapons, electrotherapy and hypnotism, for prescribing a visit to a hydropathic establishment after a single consultation was an inadequate source of income. My knowledge of electrotherapy was derived from W. Erb's textbook, which provided detailed instructions for the treatment of all the symptoms of nervous diseases. Unluckily I was soon driven to see that following these instructions was of no help whatever and that what I had taken for an epitome of exact observations was merely the construction of phantasy. The realization that the work of the greatest name in German neuropathology had no more relation to reality than some "Egyptian" dream-book, such as is sold in cheap bookshops, was painful, but it helped to rid me of another shred of the

innocent faith in authority from which I was not yet free. So I put my electrical apparatus aside, even before Möbius had solved the problem by explaining that the successes of electric treatment in nervous disorders (in so far as there were any) were the effect of suggestion on the part of the physician.

With hypnotism the case was better. While I was still a student I had attended a public exhibition given by Hansen the "magnetist," and had noticed that one of the persons experimented upon had become deathly pale at the onset of cataleptic rigidity and had remained so as long as that condition lasted. This firmly convinced me of the genuineness of the phenomena of hypnosis. Scientific support was soon afterwards given to this view by Heidenhain; but that did not restrain the professors of psychiatry from declaring for a long time to come that hypnotism was not only fraudulent but dangerous and from regarding hypnotists with contempt. In Paris I had seen hypnotism used freely

as a method for producing symptoms in patients and then removing them again. And now the news reached us that a school had arisen at Nancy which made an extensive and remarkably successful use of suggestion, with or without hypnosis, for therapeutic purposes. It thus came about, as a matter of course, that in the first years of my activity as a physician my principal instrument of work, apart from haphazard and unsystematic psycho-therapeutic methods, was hypnotic suggestion.

This implied, of course, that I abandoned the treatment of organic nervous diseases; but that was of little importance. For on the one hand the prospects in the treatment of such disorders were in any case never promising, while on the other hand, in the private practice of a physician working in a large town, the quantity of such patients was nothing compared to the crowds of neurotics, whose number seemed further multiplied by the manner in which they hurried, with their troubles unsolved, from one physician to an-

other. And apart from this, there was something positively seductive in working with hypnotism. For the first time there was a sense of having overcome one's helplessnesss; and it was highly flattering to enjoy the reputation of being a miracle-worker. It was not until later that I was to discover the drawbacks of the procedure. At the moment there were only two points to complain of: first, that I could not succeed in hypnotizing every patient, and secondly, that I was unable to put individual patients into as deep a state of hypnosis as I should have wished. With the idea of perfecting my hypnotic technique, I made a journey to Nancy in the summer of 1889 and spent several weeks there. I witnessed the moving spectacle of old Liébault working among the poor women and children of the laboring classes; I was a spectator of Bernheim's astonishing experiments upon his hospital patients; and I received the profoundest impression of the possibility that there could be powerful mental processes which nevertheless remained hidden

from the consciousness of men. Thinking it would be instructive, I had persuaded one of my patients to follow me to Nancy. She was a very highly gifted hysteric, a woman of good birth, who had been handed over to me because no one knew what to do with her. By hypnotic influence I had made it possible for her to lead a tolerable existence and I was always able to take her out of the misery of her condition. But she always relapsed again after a short time, and in my ignorance I attributed this to the fact that her hypnosis had never reached the stage of somnambulism with amnesia. Bernheim now attempted several times to bring this about, but he, too, failed. He frankly admitted to me that his great therapeutic successes by means of suggestion were only achieved in his hospital practice and not with his private patients. I had many stimulating conversations with him, and undertook to translate into German his two works upon suggestion and its therapeutic effects.

During the period from 1886 to 1891 I did little scientific work, and published scarcely anything. I was occupied with establishing myself in my new profession and with assuring my own material existence as well as that of a rapidly increasing family. In 1891 there appeared the first of my studies upon the cerebral paralyses of children, which was written in collaboration with my friend and assistant, Dr. Oskar Rie. An invitation which I received in the same year to contribute to an encyclopaedia of medicine led me to investigate the theory of aphasia, which was at that time dominated by the views of Wernicke and Lichtheim, which laid stress exclusively upon localization. The fruit of this inquiry was a small critical and speculative book, *Zur Auffassung der Aphasie.* But I must now show how it happened that scientific research once more became the chief interest of my life.

II

I MUST supplement what I have just said by explaining that from the very first I made use of hypnosis in another manner, apart from hypnotic suggestion. I used it for questioning the patient upon the origin of his symptom, which in his waking state he could often describe only very imperfectly or not at all. Not only did this method seem more effective than bald suggestive commands or prohibitions, but it also satisfied the curiosity of the physician, who, after all, had a right to learn something of the origin of the phenomenon which he was striving to remove by the monotonous procedure of suggestion.

The manner in which I arrived at this other procedure was as follows. While I was still working in Brücke's laboratory I had made the acquaintance of Dr. Josef Breuer, who was one

of the most respected family physicians in Vienna, but who also had a scientific past, since he had produced several works of permanent value upon the physiology of breathing and upon the organ of equilibrium. He was a man of striking intelligence and fourteen years older than myself. Our relations soon became more intimate and he became my friend and helper in my difficult circumstances. We grew accustomed to share all our scientific interests with each other. In this relationship the gain was naturally mine. The development of psychoanalysis afterwards cost me his friendship. It was not easy for me to pay such a price, but I could not escape it.

Even before I went to Paris, Breuer had told me about a case of hysteria which, between 1880 and 1882, he had treated in a peculiar manner which had allowed him to penetrate deeply into the causation and significance of hysterical symptoms. This was at a time, therefore, when Janet's works still belonged to the future. He repeatedly

read me pieces of the case history, and I had an impression that it accomplished more towards an understanding of neuroses than any previous observation. I determined to inform Charcot of these discoveries when I reached Paris, and I actually did so. But the great man showed no interest in my first outline of the subject, so that I never returned to it and allowed it to pass from my mind.

When I was back in Vienna I turned once more to Breuer's observation and made him tell me more about it. The patient had been a young girl of unusual education and gifts, who had fallen ill while she was nursing her father, of whom she was devotedly fond. When Breuer took over her case it presented a variegated picture of paralyses with contractures, inhibitions and states of mental confusion. A chance observation showed her physician that she could be relieved of these clouded states of consciousness if she was induced to express in words the affective phantasy by which she was at the moment

dominated. From this discovery, Breuer arrived at a new method of treatment. He put her into deep hypnosis and made her tell him each time what it was that was oppressing her mind. After the attacks of depressive confusion had been overcome in this way, he employed the same procedure for removing her inhibitions and physical disorders. In her waking state the girl could no more describe than other patients how her symptoms had arisen, and she could discover no link between them and any experiences of her life. In hypnosis she immediately revealed the missing connection. It turned out that all her symptoms went back to moving events which she had experienced while nursing her father; that is to say, her symptoms had a meaning and were residues or reminiscences of those emotional situations. It turned out in most instances that there had been some thought or impulse which she had had to suppress while she was by her father's sick-bed, and that, in place of it, as a substitute for it, the symptom had afterwards

appeared. But as a rule the symptom was not the precipitate of a single such "traumatic" scene, but the result of a summation of a number of similar situations. When the patient recalled a situation of this kind in a hallucinatory way under hypnosis and carried through to its conclusion, with a free expression of emotion, the mental act which she had originally suppressed, the symptom was abolished and did not return. By this procedure Breuer succeeded, after long and painful efforts, in relieving his patient of all her symptoms.

The patient had recovered and had remained well and, in fact, had become capable of doing serious work. But over the final stage of this hypnotic treatment there rested a veil of obscurity, which Breuer never raised for me; and I could not understand why he had so long kept secret what seemed to me an invaluable discovery instead of making science the richer by it. The immediate question, however, was whether it was possible to generalize from what he had

found in a single case. The state of things which he had discovered seemed to me to be of so fundamental a nature that I could not believe it could fail to be present in any case of hysteria if it had been proved to occur in a single one. But the question could only be decided by experience. I therefore began to repeat Breuer's investigations with my own patients and eventually, especially after my visit to Bernheim in 1889 had taught me the limitations of hypnotic suggestion, I worked at nothing else. After observing for several years that his findings were invariably confirmed in every case of hysteria that was accessible to such treatment, and after having accumulated a considerable amount of material in the shape of observations analogous to his, I proposed to him that we should issue a joint publication. At first he objected vehemently, but in the end he gave way, especially since, in the meantime, Janet's works had anticipated some of his results, such as the tracing back of hysterical symptoms to events in the patient's life,

and their removal by means of hypnotic reproduction *in statu nascendi*. In 1893 we issued a preliminary paper, "On the Psychical Mechanism of Hysterical Phenomena," [1] and in 1895 there followed our book, *Studien über Hysterie*.

If the account I have so far given has led the reader to expect that the *Studien über Hysterie* must, in all the essentials of their material content, be the product of Breuer's mind, that is precisely what I myself have always maintained and what it has been my aim to repeat here. As regards the *theory* put forward in the book, I was partly responsible, but to an extent which it is today no longer possible to determine. That theory was in any case unpretentious and hardly went beyond the direct description of the observations. It did not seek to establish the nature of hysteria but merely to throw light upon the origin of its symptoms. Thus it laid stress upon the significance of the life of the emotions and upon the importance of distinguishing between

[1] [Freud, *Collected Papers*, Vol. 1.]

mental acts which are unconscious and those which are conscious (or rather capable of being conscious); it introduced a dynamic factor, by supposing that a symptom arises through the damming-up of an affect, and an economic factor, by regarding that same symptom as the product or equivalent of a quantity of energy which would otherwise have been employed in some other way. (This latter process was described as *conversion.*) Breuer spoke of our method as *cathartic;* its therapeutic aim was explained as being to provide that the accumulated affect used for maintaining the symptom, which had got on to the wrong lines and had, as it were, become stuck there, should be directed on to the normal path along which it could obtain discharge (or *abreaction*). The practical results of the cathartic procedure were excellent. Its defects, which became evident later, were those of all forms of hypnotic treatment. There are still a number of psychotherapists who have not gone beyond catharsis as Breuer understood it

and who still speak in its favor. Its value as an abridged method of treatment was shown afresh in the hands of Simmel in the treatment of war neuroses in the German army during the Great War. The theory of catharsis had not much to say on the subject of sexuality. In the case histories which I contributed to the *Studien,* sexual factors played a certain part, but scarcely more attention was paid to them than to other emotional excitations. Breuer wrote of the girl, who has since become famous as his first patient, that her sexual side was extraordinarily undeveloped. It would have been difficult to guess from the *Studien über Hysterie* what an importance sexuality has in the aetiology of the neuroses.

The stage of development which now followed, the transition from catharsis to psychoanalysis proper, has been described by me several times already in such detail that I shall find it difficult to bring forward any new facts. The event which formed the opening of this period was Breuer's retirement from our common work,

so that I became the sole administrator of his legacy. There had been differences of opinion between us at quite an early stage, but they had not been a ground for our separating. In answering the question of when it is that a mental process becomes pathogenic, that is, when it is that it becomes impossible for it to find a normal discharge, Breuer preferred what might be called a physiological theory: he thought that the processes which could not find a normal outcome were such as had originated during unusual, "hypnoid," mental states. This opened the further question of the origin of these hypnoid states. I, on the other hand, was inclined to suspect the existence of an interplay of forces and the operation of intentions and purposes such as are to be observed in normal life. Thus it was a case of "Hypnoid Hysteria" versus "Defence Neurosis." But such differences as this would scarcely have alienated him from the subject if there had not been other factors at work. One of these was undoubtedly that his

work as a physician and family doctor took up much of his time and that he could not, like me, devote his whole strength to the work of catharsis. Again, he was affected by the reception which our book had received both in Vienna and in Germany. His self-confidence and powers of resistance were not developed so fully as the rest of his mental organization. When, for instance, the *Studien* met with a severe rebuff from Strümpell, I was able to laugh at the lack of comprehension which his criticism showed, but Breuer felt hurt and grew discouraged. But what contributed chiefly to his decision was that my own further work led in a direction to which he found it impossible to reconcile himself.

The theory which we had attempted to construct in the *Studien* remained, as I have said, very incomplete; and in particular we had scarcely touched upon the problem of aetiology, upon the question of the ground in which the pathogenic process takes root. I now learned from my rapidly increasing experience that it

was not *any* kind of emotional excitation that was in action behind the phenomena of the neurosis but habitually one of a sexual nature, whether it was a current sexual conflict or the effect of earlier sexual experiences. I was not prepared for this conclusion and my expectations played no part in it, for I had begun my investigation of neurotics quite unsuspectingly. While I was writing my "History of the Psycho-Analytic Movement" in 1914, there recurred to my mind some remarks that had been made to me by Breuer, Charcot and Chrobak, which might have led me to this discovery earlier. But at the time I heard them I did not understand what these authorities meant; indeed they had told me more than they knew themselves or were prepared to defend. What I heard from them lay dormant and passive within me, until the chance of my cathartic experiments brought it out as an apparently original discovery. Nor was I then aware that in deriving hysteria from sexuality I was going back to the very beginnings of medicine

and following up a thought of Plato's. It was not until later that I learned this from an essay by Havelock Ellis.

Under the influence of my surprising discovery, I now took a momentous step. I went beyond the domain of hysteria and began to investigate the sexual life of the so-called neurasthenics who used to visit me in numbers during my consultation hours. This experiment cost me, it is true, my popularity as a doctor, but it brought me convictions which today, almost thirty years later, have lost none of their force. There was a great deal of equivocation and mystery-making to be overcome, but, once that had been done, it turned out that in all of these patients grave abuses of the sexual function were present. Considering how extremely widespread are these abuses on the one hand and neurasthenia on the other, a frequent coincidence between the two would not have proved much; but there was more in it than that one bald fact. Closer observation suggested to me that it was

possible to pick out from the confused jumble of clinical pictures covered by the name of neurasthenia two fundamentally different types, which might appear in any degree of mixture but which were nevertheless to be observed in their pure forms. In the one type the central phenomenon was the anxiety attack with its equivalents, rudimentary forms and chronic surrogate symptoms; I consequently gave it the name of *anxiety neurosis,* and limited the term *neurasthenia* to the other type. Now it was easy to establish the fact that each of these types had a different abnormality of sexual life as its corresponding aetiological factor: in the former, *coitus interruptus,* undischarged excitement and sexual abstinence, and in the latter, excessive masturbation and too numerous nocturnal emissions. In a few specially instructive cases, which had shown a surprising alteration in the clinical picture from one type to the other, it was possible to prove that there had been a corresponding change in the underlying sexual *régime.* If it was

possible to put an end to the abuse and allow its place to be taken by normal sexual activity, a striking improvement in the condition was the reward.

I was thus led into regarding the neuroses as being without exception disturbances of the sexual function, the so-called "actual" neuroses being the direct toxic expression of such disturbances and the psycho-neuroses their mental expression. My medical conscience felt pleased at my having arrived at this conclusion. I hoped that I had filled up a gap in medical science, which, in dealing with a function of such great biological importance, had failed to take into account any injuries beyond those caused by infection or by gross anatomical lesions. The medical aspect of the matter was, moreover, supported by the fact that sexuality was not something purely mental. It had a somatic side as well, and it was possible to assign special chemical processes to it and to attribute sexual excitement to the presence of some particular,

though at present unknown, substances. There must also have been some good reason why the true spontaneous neuroses resembled no group of diseases more closely than the phenomena of intoxication and abstinence, which are produced by the administration or privation of certain toxic substances, or than exophthalmic goiter, which is known to depend upon the product of the thyroid gland.

Since that time I have had no opportunity of returning to the investigation of the actual neuroses; nor has this part of my work been continued by anyone else. If I look back today at my early findings, they strike me as being the first rough outlines of what is probably a far more complicated subject. But on the whole they seem to me still to hold good. I should have been very glad if I had been able, later on, to make a psycho-analytic examination of some more cases of simple juvenile neurasthenia, but unluckily the occasion did not arise. To avoid misconceptions, I should like to make it clear

that I am far from denying the existence of mental conflicts and of neurotic complexes in neurasthenia. All that I am asserting is that the symptoms of these patients are not mentally determined or removable by analysis, but that they must be regarded as direct toxic consequences of disturbed sexual chemical processes.

During the years that followed the publication of the *Studien,* having reached these conclusions upon the part played by sexuality in the aetiology of the neuroses, I read some papers on the subject before various medical societies, but was only met with incredulity and contradiction. Breuer did what he could for some time longer to throw the great weight of his personal influence into the scales in my favor, but he effected nothing and it was easy to see that he, too, shrank from recognizing the sexual aetiology of the neuroses. He might have crushed me or at least disconcerted me by pointing to his own first patient, in whose case sexual factors had ostensibly played no part whatever. But he never

did so, and I could not understand why this was until I came to interpret the case correctly and to reconstruct, from some remarks which he had made, the conclusion of his treatment of it. After the work of catharsis had seemed to be completed, the girl had suddenly developed a condition of "transference love"; he had not connected this with her illness, and had therefore retired in dismay. It was obviously painful to him to be reminded of this apparent *contre-temps*. His attitude towards me oscillated for some time between appreciation and bitter criticism; then accidental difficulties arose, as they never fail to do in a strained situation, and we parted.

Another result of my taking up the study of nervous disorders in general was that I altered the technique of catharsis. I abandoned hypnotism and sought to replace it by some other method, because I was anxious not to be restricted to treating hysteriform conditions. Increasing experience had also given rise to two

grave doubts in my mind as to the use of hypnotism even as a means to catharsis. The first was that even the most brilliant results were liable to be suddenly wiped away if my personal relation with the patient became disturbed. It was true that they would be re-established if a reconciliation could be effected; but such an occurrence proved that the personal emotional relation between doctor and patient was after all stronger than the whole cathartic process, and it was precisely that factor which escaped every effort at control. And one day I had an experience which showed me in the crudest light what I had long suspected. One of my most acquiescent patients, with whom hypnotism had enabled me to bring about the most marvellous results, and whom I was engaged in relieving of her suffering by tracing back her attacks of pain to their origins, as she woke up on one occasion threw her arms round my neck. The unexpected entrance of a servant relieved us from a painful discussion, but from that time onwards there was a tacit

understanding between us that the hypnotic treatment should be discontinued. I was modest enough not to attribute the event to my own irresistible personal attraction, and I felt that I had now grasped the nature of the mysterious element that was at work behind hypnotism. In order to exclude it, or at all events to isolate it, it was necessary to abandon hypnotism.

But hypnotism had been of immense help in the cathartic treatment, by widening the field of the patient's consciousness and putting within his reach knowledge which he did not possess in his waking life. It seemed no easy task to find a substitute for it. While I was in this perplexity, there came to my help the recollection of an experiment which I had often witnessed while I was with Bernheim. When the subject awoke from the state of somnambulism, he seemed to have lost all memory of what had happened while he was in that state. But Bernheim maintained that the memory was present all the same; and if he insisted on the subject remembering,

if he asseverated that the subject knew it all and had only to say it, and if at the same time he laid his hand on the subject's forehead, then the forgotten memories used in fact to return, hesitatingly at first, but eventually in a flood and with complete clarity. I determined that I would act in the same way. My patients, I reflected, must in fact "know" all the things which had hitherto only been made accessible to them in hypnosis; and assurances and encouragement on my part, assisted perhaps by the touch of my hand, would, I thought, have the power of forcing the forgotten facts and connections into consciousness. No doubt this seemed a more laborious process than putting them into hypnosis, but it might prove highly instructive. So I abandoned hypnotism, only retaining my practice of requiring the patient to lie upon a sofa while I sat behind him, seeing him, but not seen myself.

III

MY expectations were fulfilled; I was set free from hypnotism. But along with the change in technique the process of catharsis took on a new complexion. Hypnosis had screened from view an interplay of forces which now came in sight and the understanding of which gave a solid foundation to my theory.

How had it come about that the patients had forgotten so many of the facts of their external and internal lives but could nevertheless recollect them if a particular technique was applied? Observation supplied an exhaustive answer to these questions. Everything that had been forgotten had in some way or other been painful; it had been either alarming or disagreeable or shameful by the standards of the subject's personality. The thought arose spontaneously: that

was precisely why it had been forgotten, i.e., why it had not remained conscious. In order to make it conscious again in spite of this, it was necessary to overcome something that fought against one in the patient; it was necessary to make an expenditure of effort on one's own part in order to compel and subdue it. The amount of effort required of the physician varied in different cases; it increased in direct proportion to the difficulty of what had to be remembered. The expenditure of force on the part of the physician was evidently the measure of a *resistance* on the part of the patient. It was only necessary to translate into words what I myself had observed, and I was in possession of the theory of *repression.*

It was now easy to reconstruct the pathogenic process. Let us keep to a simple example, in which a particular impulsion had arisen in the subject's mind but was opposed by other powerful tendencies. We should have expected the mental *conflict* which now arose to take the fol-

lowing course: The two dynamic quantities—for our present purposes let us call them "the instinct" and "the resistance"—would struggle with each other for some time in the fullest light of consciousness, until the instinct was repudiated and the *charge*[1] *of energy* withdrawn from it. This would have been the normal solution. In a neurosis, however, (for reasons which were still unknown) the conflict found a different outcome. The ego drew back, as it were, after the first shock of its conflict with the objectionable impulse; it debarred the impulse from access to consciousness and to direct motor discharge, but at the same time the impulse retained its full charge of energy. I named this process *repression;* it was a novelty, and nothing like it had ever before been recognized in mental life. It

[1] [The German word *Besetzung*, here translated "charge," is applied by Freud to the sum of energy which he supposes to become attached (somewhat upon the analogy of an electric charge) to mental impulses, whether conscious or unconscious, when they are in a condition of activity. The recognized English technical translation of the word is *cathexis.—Trans.*]

was obviously a primary mechanism of defence, comparable to an attempt at flight, and was only a forerunner of the later-developed normal condemning judgment. The first act of repression involved further consequences. In the first place, the ego was obliged to protect itself against the constant threat of a renewed advance on the part of the repressed impulse by making a permanent expenditure of energy, a *counter-charge* or *anti-cathexis,* and it thus impoverished itself. On the other hand, the repressed impulse, which was now *unconscious,* was able to find means of discharge and of substitutive gratification by circuitous routes and thus bring the whole purpose of the repression to nothing. In the case of conversion-hysteria the circuitous route led to the nerve supply of the body; the repressed impulse broke its way through at some point or other and produced *symptoms.* The symptoms were thus results of a compromise, for although they were substitutive gratifications they were nevertheless distorted and deflected

from their aim, owing to the resistance of the ego.

The theory of repression became the foundation-stone of our understanding of the neuroses. A different view had now to be taken of the task of therapy. Its aim was no longer to "abreact" an affect which had got on to the wrong lines, but to uncover repressions and replace them by acts of judgment which might result either in the acceptance or in the rejection of what had formerly been repudiated. I showed my recognition of the new situation by no longer calling my method of investigation and treatment catharsis but *psycho-analysis.*

It is possible to take repression as a center and to bring all the elements of psycho-analytical theory into relation with it. But before doing so I have a further remark of a polemical nature to make. According to Janet's view a hysterical woman was a wretched creature who, on account of a constitutional weakness, was unable to hold her mental acts together, and it was for

that reason that she fell a victim to mental dissociation and to a restriction of the field of her consciousness. The results of psycho-analytical investigations, on the other hand, showed that these phenomena were the result of dynamic factors—of mental conflict and of repression. This distinction seems to me to be far-reaching enough to put an end to the glib repetition of the view that whatever is of value in psycho-analysis is merely borrowed from the ideas of Janet. The reader will have learned from my account that historically psycho-analysis is completely independent of Janet's discoveries, just as in its content it diverges from them and goes far beyond them. Janet's works would never have had the implications which have made psycho-analysis of such importance to the mental sciences and have made it attract such universal interest. I always treated Janet himself with respect, since his discoveries coincided to a considerable extent with those of Breuer, which had been made earlier but were published later than

his. But when in the course of time psycho-analysis became a subject of discussion in France, Janet behaved ill, showed ignorance of the facts and used ugly arguments. And finally he revealed himself to my eyes and destroyed the value of his own work by declaring that when he had spoken of "unconscious" mental acts he had meant nothing by the phrase—it had been no more than a *"façon de parler."*

But the study of pathogenic repressions and of other phenomena which have still to be mentioned compelled psycho-analysis to take the concept of the "unconscious" seriously. Psycho-analysis regarded everything mental as being in the first instance unconscious; the further quality of "consciousness" might also be present, or again it might be absent. This of course provoked a denial from the philosophers, for whom "conscious" and "mental" were identical, and who protested that they could not conceive of such a monstrosity as the "unconscious mental." There was no help for it, however, and this

idiosyncrasy of the philosophers could only be disregarded with a shrug. Experience (gained from pathological material, of which the philosophers were ignorant) of the frequency and power of impulses of which one knew nothing directly and whose existence had to be inferred like some fact in the external world, left no alternative open. It could be pointed out, incidentally, that this was only treating one's own mental life as one had always treated other people's. One did not hesitate to ascribe mental processes to other people, although one had no immediate consciousness of them and could only infer them from their words and actions. But what held good for other people must be applicable to oneself. Anyone who tried to push the argument further and to conclude from it that one's own hidden processes belonged actually to a second *consciousness* would be faced with the concept of a consciousness of which one knew nothing, of an "unconscious consciousness"—and this would scarcely be preferable to

the assumption of an "unconscious mental." If, on the other hand, one declared, like some other philosophers, that one was prepared to take pathological phenomena into account, but that the processes underlying them ought not to be described as mental but as "psychoid," the difference of opinion would degenerate into an unfruitful dispute about words, though even so expediency would decide in favor of keeping the expression "unconscious mental." The further question as to the ultimate nature of this unconscious is no wiser or more profitable than the older one as to the nature of the conscious.

It would be more difficult to explain concisely how it came about that psycho-analysis made a further distinction in the unconscious, and separated it into a *preconscious* and an unconscious proper. It will be sufficient to say that it appeared a legitimate course to supplement the theories which were a direct expression of experience by hypotheses which were designed to facilitate the handling of the material and re-

lated to matters which could not be a subject of immediate observation. The very same procedure is adopted by the older sciences. The subdivision of the unconscious is part of an attempt to picture the apparatus of the mind as being built up of a number of *instances* or systems whose inter-relations may be expressed in spatial terms, without reference, of course, to the actual anatomy of the brain. (I have described this as the *topographical* method of approach.) Such ideas as these are part of a speculative superstructure of psycho-analysis, any portion of which can be abandoned or changed without loss or regret the moment its inadequacy has been proved. But there is still plenty to be described that lies closer to actual experience.

I have already mentioned that my investigation of the precipitating and underlying causes of the neuroses led me more and more frequently to conflicts between the subject's sexual impulses and his resistance to sexuality. In my search for the pathogenic situations in which the repres-

sions of sexuality had set in and in which the symptoms, as substitutes for what was repressed, had had their origin, I was carried further and further back into the patient's life and ended by reaching the first years of his childhood. What poets and students of human nature had always asserted turned out to be true: the impressions of that remote period of life, though they were for the most part buried in amnesia, left ineradicable traces upon the individual's growth and in particular laid the foundations of any nervous disorder that was to follow. But since these experiences of childhood were always concerned with sexual excitations and the reaction against them, I found myself faced by the fact of infantile sexuality—once again a novelty and a contradiction of one of the strongest of human prejudices. Childhood was looked upon as "innocent" and free from the lusts of sex, and the fight with the demon of "sensuality" was not thought to begin until the troubled age of puberty. Such occasional sexual activities as it

had been impossible to overlook in children were put down as signs of degeneracy and premature depravity or as a curious freak of nature. Few of the findings of psycho-analysis have met with such universal contradiction or have aroused such an outburst of indignation as the assertion that the sexual function starts at the beginning of life and reveals its presence by important signs even in childhood. And yet no other finding of analysis can be demonstrated so easily and so completely.

Before going further into the question of infantile sexuality I must mention an error into which I fell for a while and which might well have had fatal consequences for the whole of my work. Under the pressure of the technical procedure which I used at that time, the majority of my patients reproduced from their childhood scenes in which they were sexually seduced by some grown-up person. With female patients the part of seducer was almost always assigned to their father. I believed these stories, and conse-

quently supposed that I had discovered the roots of the subsequent neurosis in these experiences of sexual seduction in childhood. My confidence was strengthened by a few cases in which relations of this kind with a father, uncle or elder brother had continued up to an age at which memory was to be trusted. If the reader feels inclined to shake his head at my credulity, I cannot altogether blame him; though I may plead that this was at a time when I was intentionally keeping my critical faculty in abeyance so as to preserve an unprejudiced and receptive attitude towards the many novelties which were coming to my notice every day. When, however, I was at last obliged to recognize that these scenes of seduction had never taken place, and that they were only phantasies which my patients had made up or which I myself had perhaps forced upon them, I was for some time completely at a loss. My confidence alike in my technique and in its results suffered a severe blow; it could not be disputed that I had arrived at these scenes by a

technical method which I considered correct, and their subject-matter was unquestionably related to the symptoms from which my investigation had started. When I had pulled myself together, I was able to draw the right conclusions from my discovery: namely, that the neurotic symptoms were not related directly to actual events but to phantasies embodying wishes, and that as far as the neurosis was concerned psychical reality was of more importance than material reality. I do not believe even now that I forced the seduction-phantasies upon my patients, that I "suggested" them. I had in fact stumbled for the first time upon the *Oedipus complex,* which was later to assume such an overwhelming importance, but which I did not recognize as yet in its disguise of phantasy. Moreover, seduction during childhood retained a certain share, though a humbler one, in the aetiology of neuroses. But the seducers turned out as a rule to have been older children.

It will be seen, then, that my mistake was of

the same kind as would be made by someone who believed that the legendary story of the early kings of Rome (as told by Livy) was historical truth instead of what it is in fact—a reaction against the memory of times and circumstances that were insignificant and occasionally, perhaps, inglorious. When the mistake had been cleared up, the path to the study of the sexual life of children lay open. It thus became possible to apply psycho-analysis to another field of science and to use its data as a means of discovering a new piece of biological knowledge.

The sexual function, as I found, is in existence from the very beginning of the individual's life, though at first it is assimilated to the other vital functions and does not become independent of them until later; it has to pass through a long and complicated process of development before it becomes what we are familiar with as the normal sexual life of the adult. It begins by manifesting itself in the activity of a whole number of *component instincts.* These are dependent

upon *erotogenic zones* in the body; some of them make their appearance in pairs of opposite impulses (such as sadism and masochism or the impulses to look and to be looked at); they operate independently of one another in their search for pleasure, and they find their object for the most part in the subject's own body. Thus to begin with they are non-centralized and predominantly *auto-erotic*. Later they begin to be co-ordinated; a first stage of organization is reached under the dominance of the *oral* components, an *anal-sadistic* stage follows, and it is only after the third stage has at last been reached that the primacy of the *genitals* is established and that the sexual function begins to serve the ends of reproduction. In the course of this process of development a number of elements of the various component instincts turn out to be unserviceable for this last end and are therefore left on one side or turned to other uses, while others are diverted from their aims and carried over into the genital organization. I gave the

name of *libido* to the energy of the sexual instincts and to that form of energy alone. I was next driven to suppose that the libido does not always pass through its prescribed course of development smoothly. As a result either of the excessive strength of certain of the components or of experiences involving premature gratification, *fixations* of the libido may occur at various points in the course of its development. If subsequently a repression takes place, the libido flows back to these points (a process described as *regression*), and it is from them that the energy breaks through in the form of a symptom. Later on it further became clear that the localization of the point of fixation is what determines the *choice of neurosis,* that is, the form in which the subsequent illness makes its appearance.

The process of arriving at an *object,* which plays such an important part in mental life, takes place alongside the organization of the libido. After the stage of *auto-erotism,* the first

love-object in the case of both sexes is the mother; and it seems probable that to begin with the child does not distinguish its mother's organ of nutrition from its own body. Later, but still in the first years of infancy, the relation known as the Oedipus complex becomes established: boys concentrate their sexual wishes upon their mother and develop hostile impulses against their father as being a rival, while girls adopt an analogous attitude.[2] All of the different variations and consequences of the Oedipus complex are important; and in particular the innately bisexual constitution of human beings makes itself felt and increases the number of simultane-

[2] (Additional Note, 1935:) The information about infantile sexuality was obtained from the study of men and the theory deduced from it was concerned with male children. It was natural enough to expect to find a complete parallel between the two sexes; but this turned out not to hold. Further investigations and reflections revealed profound differences between the sexual development of men and women. The first sexual object of a baby girl (just as of a baby boy) is her mother; and before a woman can reach the end of her normal development she has to change not only her sexual object but also her predominant genital zone. From this circumstance difficulties arise and possibilities of inhibition which are not present in the case of men.

ously active tendencies. Children do not become clear for quite a long time upon the differences between the sexes; and during this period of *sexual inquiry* they produce typical *sexual theories* which, since they are limited by the incompleteness of their authors' own physical development, are a mixture of truth and error and fail to solve the problems of sexual life (the riddle of the Sphinx—the question of where babies come from). We see, then, that a child's first object-choice is an *incestuous* one. The whole course of development that I have described is run through rapidly. For the most remarkable feature of the sexual life of man is that it comes on in two waves, with an interval between them. It reaches a first maximum in the fourth or fifth year of a child's life. But this early growth of sexuality is nipped in the bud; the sexual impulses which have shown such liveliness are overcome by repression, and a *period of latency* follows, which lasts until puberty and during which the *reaction-formations* of morality, shame

and disgust, are built up.[3] Of all living creatures man alone seems to show this double onset of sexual growth, and it may perhaps be the biological determinant of his predisposition to neuroses. At puberty the impulses and object-relations of a child's early years become reanimated, and amongst them the emotional ties of his Oedipus complex. The sexual life of puberty is a struggle between the impulses of early years and the inhibitions of the latency period. Before this, and while the child is at the highest point of its infantile sexual development, a genital organization of a sort is established; but only the male genitals play a part in it, and the female ones remain undiscovered. (I have described this as the period of *phallic* primacy.) At this stage the contrast between the sexes is not stated in terms of "male" or "female" but of

[3] (Additional Note, 1935:) The period of latency is a physiological phenomenon. It can, however, only give rise to a complete interruption of sexual life in cultural organizations which have made the suppression of infantile sexuality a part of their system. This is not the case with the majority of primitive peoples.

"possessing a penis" or "castrated." The *castration complex* which arises in this connection is of the profoundest importance in the formation alike of character and of neuroses.

In order to make this condensed account of my discoveries upon the sexual life of man more intelligible, I have brought together conclusions which I reached at different dates and incorporated by way of supplement or correction in the successive editions of my *Three Contributions to the Theory of Sexuality.*[4] I hope it will have been easy to gather the nature of my extension (on which so much stress has been laid and which has excited so much opposition) of the concept of sexuality. That extension is of a two-fold kind. In the first place sexuality is divorced from its too close connection with the genitals and is regarded as a more comprehensive bodily function, having pleasure as its goal and only secondarily coming to serve the ends

[4] [First German edition, under the title of *Drei Abhandlungen zur Sexualtheorie,* Vienna, 1905.]

of reproduction. In the second place the sexual impulses are regarded as including all of those merely affectionate and friendly impulses to which usage applies the exceedingly ambiguous word "love." I do not, however, consider that these extensions are innovations, but rather restorations: they signify the removal of inexpedient limitations of the concept into which we had allowed ourselves to be led.

The detaching of sexuality from the genitals has the advantage of allowing us to bring the sexual activities of children and of perverts into the same scope as those of normal adults. The former have hitherto been entirely neglected and though the latter have been recognized it has been with moral indignation and without understanding. Looked at from the psycho-analytic standpoint, even the most eccentric and repellent perversions are explicable as manifestations of component instincts of sexuality which have freed themselves from the primacy of the genitals and are going in pursuit of pleasure on their

own account as they did in the very early days of the libido's development. The most important of these perversions, homosexuality, scarcely deserves the name. It can be traced back to the constitutional bisexuality of all human beings and to the after-effects of the phallic primacy. Psycho-analysis enables us to point to some trace or other of a homosexual object-choice in everyone. If I have described children as "polymorphously perverse," I was only using a terminology that was generally current; no moral judgment was implied by the phrase. Psycho-analysis has no concern whatever with such judgments of value.

The second of my alleged extensions of the concept of sexuality finds its justification in the fact revealed by psycho-analytic investigation that all of these affectionate impulses were originally of a completely sexual nature but have become *inhibited in their aim* or *sublimated.* The manner in which the sexual instincts can thus be influenced and diverted enables them to

be employed for cultural activities of every kind, to which indeed they bring the most important contributions.

My surprising discoveries as to the sexuality of children were made in the first instance through the analysis of adults. But later (from about 1908 onwards) it became possible to confirm them in the most satisfactory way and in every detail by direct observations upon children. Indeed, it is so easy to convince oneself of the regular sexual activities of children that one cannot help asking in astonishment how the human race can have succeeded in overlooking the facts, and in maintaining for so long the agreeable legend of the asexuality of childhood. This surprising circumstance must be connected with the amnesia which with the majority of adults hides their own infancy.

IV

THE theories of resistance and of repression, of the unconscious, of the aetiological significance of sexual life and of the importance of infantile experiences—these form the principal constituents of the theoretical structure of psycho-analysis. In these pages, unfortunately, I have been able to describe only the separate elements and not their inter-connections and their bearing upon one another. But I am obliged now to turn to the alterations which gradually took place in the technique of the analytic method.

The means which I first adopted for overcoming the patient's resistance, by pressing and encouraging him, had been indispensable for the purpose of giving me a first general survey of what was to be expected. But in the long run it proved to be too much of a strain upon both

sides, and further it seemed open to certain obvious criticisms. It therefore gave place to another method which was in one sense its opposite. Instead of urging the patient to say something upon some particular subject, I now asked him to abandon himself to a process of *free association,* i.e., to say whatever came into his head, while ceasing to give any conscious direction to his thoughts. It was essential, however, that he should bind himself to report literally everything that occurred to his self-perception, and not to give way to critical objections which sought to put certain associations on one side on the ground that they were not sufficiently important or that they were irrelevant or that they were altogether meaningless. There was no necessity to repeat explicitly the insistence upon the need for candor on the patient's part in reporting his thoughts, for it was the pre-condition of the whole analytic treatment.

It may seem surprising that this method of free association, carried out subject to the ob-

servation of *the fundamental rule of psycho-analysis,* should have achieved what was expected of it, namely, the bringing into consciousness of the repressed material which was held back by resistances. We must, however, bear in mind that free association is not really free. The patient remains under the influence of the analytic situation even though he is not directing his mental activities on to a particular subject. We shall be justified in assuming that nothing will occur to him that has not some reference to that situation. His resistance against reproducing the repressed material will now be expressed in two ways. Firstly, it will be shown by critical objections; and it was to deal with these that the fundamental rule of psycho-analysis was invented. But if the patient observes that rule and so overcomes his reticences, the resistance will find another means of expression. It will so arrange it that the repressed material itself will never occur to the patient but only something which approximates to it in an allusive way; and the

greater the resistance, the more remote will be the substitutive association which the patient has to report from the actual idea that the analyst is in search of. The analyst, who listens composedly but without any constrained effort to the stream of associations and who, from his experience, has a general notion of what to expect, can make use of the material brought to light by the patient according to two possibilities. If the resistance is slight he will be able from the patient's allusions to infer the unconscious material itself; or if the resistance is stronger he will be able to recognize its character from the associations, as they seem to become more remote from the subject, and will explain it to the patient. Uncovering the resistance, however, is the first step towards overcoming it. Thus the work of analysis involves an *art of interpretation,* the successful handling of which may require tact and practice but which is not hard to acquire. But it is not only in the saving of labor that the method of free association has an advantage over the earlier

method. It exposes the patient to the least possible amount of compulsion; it never allows of contact being lost with the actual, current situation; it guarantees to a great extent that no factor in the structure of the neurosis will be overlooked and that nothing will be introduced into it by the expectations of the analyst. It is left to the patient in all essentials to determine the course of the analysis and the arrangement of the material; any systematic handling of particular symptoms or complexes thus becomes impossible. In complete contrast to what happened with hypnotism and with the urging method, inter-related material makes its appearance at different times and at different points in the treatment. To a spectator, therefore—though in fact there can be none—an analytic treatment would seem completely obscure.

Another advantage of the method is that it need never break down. It must theoretically always be possible to have an association, provided that no conditions are made as to its char-

acter. Yet there is one case in which in fact a breakdown occurs with absolute regularity; from its very uniqueness, however, this case, too, can be interpreted.

I now come to the description of a factor which adds an essential feature to my picture of analysis and which can claim, alike technically and theoretically, to be regarded as of the first importance. In every analytic treatment there arises, without the physician's agency, an intense emotional relationship between the patient and the analyst which is not to be accounted for by the actual situation. It can be of a positive or of a negative character and can vary between the extremes of a passionate, completely sensual love and the unbridled expression of an embittered defiance and hatred. This *transference*—to give it its shortened name—soon replaces in the patient's mind the desire to be cured, and, so long as it is affectionate and moderate, becomes the agent of the physician's influence and neither more nor less than the mainspring of the joint

work of analysis. Later on, when it has become passionate or has been converted into hostility, it becomes the principal tool of the resistance. It may then happen that it will paralyze the patient's powers of associating and endanger the success of the treatment. Yet it would be senseless to try to evade it; for an analysis without transference is an impossibility. It must not be supposed, however, that transference is created by analysis and does not occur apart from it. Transference is merely uncovered and isolated by analysis. It is a universal phenomenon of the human mind; it decides the success of all medical influence and in fact dominates the whole of each person's relations to his human environment. We can easily recognize it as the same dynamic factor that the hypnotists have named "suggestibility," which is the agent of hypnotic *rapport* and the incalculable behavior of which led to such difficulties with the cathartic method. When there is no inclination to a transference of emotion such as this, or when it has become

entirely negative, as happens in dementia praecox or paranoia, then there is also no possibility of influencing the patient by psychological means.

It is perfectly true that psycho-analysis, like other psycho-therapeutic methods, employs the instrument of suggestion (or transference). But the difference is this: that in analysis it is not allowed to play the decisive part in determining the therapeutic results. It is used instead to induce the patient to perform a piece of mental work—the overcoming of his transference-resistances—which involves a permanent alteration in his mental economy. The transference is made conscious to the patient by the analyst, and it is resolved by convincing him that in his transference-attitude he is *re-experiencing* emotional relations which had their origin in his earliest object-attachments during the repressed period of his childhood. In this way the transference is changed from the strongest weapon of the resistance into the best instrument of the analytic treatment.

Nevertheless its handling remains the most difficult as well as the most important part of the technique of analysis.

With the help of the method of free association and of the closely related art of interpretation, psycho-analysis succeeded in achieving one thing which appeared to be of no practical importance but which in fact necessarily led to a fresh attitude and a fresh scale of values in scientific thought. It became possible to prove that dreams have a meaning and to discover it. In classical antiquity great importance was attached to dreams as foretelling the future; but modern science would have nothing to do with them, it handed them over to superstition, declaring them to be purely "somatic" processes—a kind of spasm occurring in a mind that is otherwise asleep. It seemed quite inconceivable that anyone who had done serious scientific work could make his appearance as an "interpreter of dreams." But by disregarding the excommunication pronounced upon dreams, by treating

them as unexplained neurotic symptoms, as delusional or obsessional ideas, by neglecting their apparent content and by making their separate component images into subjects for free association, psycho-analysis arrived at a different conclusion. The numerous associations produced by the dreamer led to the discovery of a mental structure which could no longer be described as absurd or confused, which was on an equality with any other product of the mind, and of which the *manifest* dream was no more than a distorted, abbreviated and misundersood translation and usually a translation into visual images. These *latent dream-thoughts* contained the meaning of the dream, while its manifest content was simply a make-believe, a façade, which could serve as a starting point for the associations but not for the interpretation.

There were now a whole series of questions to be answered, among the most important of them being whether there was a motive for the formation of dreams, under what conditions it

took place, by what methods the dream-thoughts (which are invariably full of sense) become converted into the dream (which is often senseless), and others besides. I attempted to solve all of these problems in *The Interpretation of Dreams,*[1] which I published in the year 1900. I can only find space here for the briefest abstract of my investigation. When the latent dream-thoughts that are revealed by the analysis of a dream are examined, one of them is found to stand out from among the rest, which are intelligible and well known to the dreamer. These latter thoughts are residues of waking life (the *day's residues,* as they are called technically); but the isolated thought is found to be an impulse in the form of a wish, often of a very repellent kind, which is foreign to the waking life of the dreamer and is consequently disavowed by him with surprise or indignation. This impulse is the actual constructor of the dream: it provides the energy for its production and makes use of

[1] [*Die Traumdeutung,* Vienna, 1900.]

the day's residues as material; the dream which thus originates represents a situation in which the impulse is satisfied; it is the fulfilment of the wish which the impulse contains. It would not be possible for this process to take place without being favored by the presence of something in the nature of a state of sleep. The necessary mental pre-condition of sleep is the concentration of the ego upon the wish to sleep and the withdrawal of psychical energy from all the interests of life; since at the same time all the paths of approach to motility are blocked, the ego is also able to reduce the expenditure of energy by which at other times it maintains the repressions. The unconscious impulse makes use of this nocturnal relaxation of repression in order to push its way into consciousness with the dream. But the repressive resistance of the ego is not abolished in sleep but merely reduced. Some of it remains in the shape of a *censorship of dreams* and forbids the unconscious impulse to express itself in the forms which it would properly

assume. In consequence of the severity of the censorship of dreams, the latent dream-thoughts are obliged to submit to being altered and softened so as to make the forbidden meaning of the dream unrecognizable. This is the explanation of *dream-distortion,* which accounts for the most striking characteristic of the manifest dream. We are therefore justified in asserting that *a dream is the (disguised) fulfilment of a (repressed) wish.* It will now be seen that dreams are constructed like a neurotic symptom: they are compromises between the demands of a repressed impulse and the resistance of a censoring force in the ego. Since they have a similar origin they are equally unintelligible and stand in equal need of interpretation.

There is no difficulty in discovering the general function of dreaming. It serves the purpose of warding off, by a kind of soothing action, external or internal stimuli which would tend to arouse the sleeper, and thus of securing sleep against interruption. External stimuli are warded

off by being given a new interpretation and by being woven into some harmless situation; internal stimuli, caused by the pressure of instincts, are given free play by the sleeper and allowed to find satisfaction in the formation of dreams, so long as the latent dream-thoughts submit to the control of the censorship. But if they threaten to break free and the meaning of the dream becomes too plain, the sleeper cuts short the dream and awakens in terror. (Dreams of this class are known as *anxiety-dreams.*) A similar failure in the function of dreaming occurs if an external stimulus becomes too strong to be warded off. (This is the class of *arousal-dreams.*) I have given the name of *dream-work* to the process which, with the co-operation of the censorship, converts the latent thoughts into the manifest content of the dream. It consists of a peculiar way of treating the preconscious material of thought, so that its component parts become *condensed,* its mental emphasis becomes *displaced,* and the whole of it is translated into

visual images or *dramatized,* and filled out by a deceptive *secondary elaboration.* The dream-work is an excellent example of the processes occurring in the deeper, unconscious layers of the mind, which differ considerably from the familiar normal processes of thought. It also displays a number of archaic characteristics, such as the use of a *symbolism* (in this case of a predominantly sexual kind) which it has since also been possible to discover in other spheres of mental activity.

We have explained that the unconscious impulse which causes the dream connects itself with part of the day's residues, with some unexhausted interest of waking life; this lends the dream which is thus brought into being a double value for the work of analysis. It is true that on the one hand a dream that has been analyzed reveals itself as the fulfilment of a repressed wish; but on the other hand it will be a continuation of some preconscious activity of the day before and will contain subject-matter of some

kind or other, whether it gives expression to a determination, a warning, a reflection, or once more to the fulfilment of a wish. Analysis exploits the dream in both directions, as a means of obtaining knowledge alike of the patient's conscious and of his unconscious processes. It also profits from the fact that dreams have access to the forgotten material of childhood, and so it happens that infantile amnesia is for the most part overcome in connection with the interpretation of dreams. In this respect dreams achieve a part of what was previously the task of hypnotism. On the other hand, I have never maintained the assertion which has so often been ascribed to me that dream-interpretation shows that all dreams have a sexual content or are derived from sexual motive forces. It is easy to see that hunger, thirst, or the need to excrete, can produce dreams of satisfaction just as well as any repressed sexual or egoistic impulse. The case of young children affords us a convenient test of the validity of our theory of dreams. In

them the various psychical systems are not yet sharply divided and the repressions have not yet grown deep, so that we often come upon dreams which are nothing more than undisguised fulfilments of impulses left over from waking life. Under the influence of imperative needs, adults may also produce dreams of this infantile type.[2]

In the same way that psycho-analysis makes use of dream-interpretation, it also profits by the study of the numerous little slips and mistakes which people make—symptomatic actions, as they are called. I investigated this subject in a series of papers which were published for the first time in book form in 1904 under the title of *The Psychopathology of Everyday Life*.[3] In this widely circulated work I have pointed out that

[2] (Additional Note, 1935:) When it is considered how frequently the function of dreaming miscarries, the dream may aptly be characterized as an *attempt* at the fulfilment of a wish. Aristotle's old definition of the dream as mental life during sleep still holds good. There was a reason for my choosing as the title of my book not *The Dream* but *The Interpretation of Dreams*.

[3] [*Zur Psychopathologie des Alltagslebens*, Berlin, 1904.]

these phenomena are not accidental, that they require more than physiological explanations, that they have a meaning and can be interpreted, and that one is justified in inferring from them the presence of restrained or repressed impulses and intentions. But what constitutes the enormous importance of dream-interpretation, as well as of this latter study, is not the assistance they give to the work of analysis, but another of their attributes. Previously psycho-analysis had only been concerned with solving pathological phenomena, and in order to explain them it had often been driven into making assumptions whose comprehensiveness was out of all proportion to the importance of the actual material under consideration. But when it came to dreams it was no longer dealing with a pathological symptom, but with a phenomenon of normal mental life which might occur in any healthy person. If dreams turned out to be constructed like symptoms, if their explanation required the same assumptions—the repression of impulses, substitute-for-

mation, compromise-formation, the dividing of the conscious and the unconscious into various psychical systems—then psycho-analysis was no longer a subsidiary science in the field of psycho-pathology; it was rather the foundation for a new and deeper science of the mind which would be equally indispensable for the understanding of the normal. Its postulates and findings could be carried over to other regions of mental happening; a path lay open to it that led far afield, into spheres of universal interest.

V

I MUST interrupt my account of the internal growth of psycho-analysis and turn to its external history. What I have so far described of its discoveries has related for the most part to the results of my own work; but I have filled in my story with material from later dates and have not distinguished between my own contributions and those of my pupils and followers.

For more than ten years after my separation from Breuer I had no followers. I was completely isolated. In Vienna I was shunned, abroad no notice was taken of me. My *Interpretation of Dreams,* published in 1900, was scarcely reviewed in the technical journals. In my paper, "On the History of the Psycho-Analytic Movement," I mentioned as an instance of the attitude adopted by psychiatric circles in Vienna a conversation with an assistant at the Clinic, who had written

a book against my theories but had never read my *Interpretation of Dreams*. He had been told at the Clinic that it was not worth while. The man in question, who has since become a professor, has gone so far as to repudiate my report of the conversation and to throw doubts in general upon the accuracy of my recollection. I can only say that I stand by every word of the account I then gave.

As soon as I realized the inevitable nature of what I had come up against, my sensitiveness greatly diminished. Moreover my isolation gradually came to an end. To begin with, a small circle of pupils gathered round me in Vienna; and then, after 1906, came the news that the psychiatrists at Zurich, E. Bleuler, his assistant C. G. Jung, and others, were taking a lively interest in psycho-analysis. We got into personal touch with one another, and at Easter, 1908, the friends of the young science met at Salzburg, agreed upon the regular repetition of similar informal congresses and arranged for the publica-

tion of a journal which was edited by Jung and was given the title of *Jahrbuch für psychopathologische und psychoanalytische Forschungen.* It was brought out under the direction of Bleuler and myself and ceased publication at the beginning of the Great War. At the same time that the Swiss psychiatrists joined the movement, interest in psycho-analysis began to be aroused all over Germany; it became the subject of a large number of written comments as well as of lively discussions at scientific congresses. But its reception was nowhere friendly or even benevolently impartial. After the briefest acquaintance with psycho-analysis German science was united in rejecting it.

Even today it is of course impossible for me to foresee the final judgment of posterity upon the value of psycho-analysis for psychiatry, psychology and the mental sciences in general. But I fancy that, when the history of the phase we have lived through comes to be written, German science will not have cause to be proud of those

who represented it. I am not thinking of the fact that they rejected psycho-analysis or of the decisive way in which they did so; both of these things were easily intelligible, they were only to be expected, and at any rate they threw no discredit upon the character of the opponents of analysis. But for the degree of arrogance which they displayed, for their conscienceless contempt of logic, and for the coarseness and bad taste of their attacks there could be no excuse. It may be said that it is childish of me to give free rein to such feelings as these now, after fifteen years have passed; nor would I do so unless I had something more to add. Years later, during the Great War, when a chorus of enemies were bringing against the German nation the charge of barbarism, a charge which sums up all that I have written above, it none the less hurt deeply to feel that my own experience would not allow me to contradict it.

One of my opponents boasted of silencing his patients as soon as they began to talk of anything

sexual, and evidently thought that this technique gave him a right to judge the part played by sexuality in the neuroses. Apart from emotional resistances, which were so easily explicable by the psycho-analytical theory that it was impossible to be misled by them, it seemed to me that the main obstacle to agreement lay in the fact that my opponents regarded psycho-analysis as a product of my speculative imagination and were unwilling to believe in the long, patient and unbiased work which had gone into its making. Since in their opinion analysis had nothing to do with observation or experience, they believed that they themselves were justified in rejecting it without experience. Others again, who did not feel so strongly convinced of this, repeated in their resistance the classical manoeuvre of not looking through the microscope so as to avoid seeing what they had denied. It is remarkable, indeed, how incorrectly most people act when they are obliged to form a judgment of their own upon some new subject. I have heard for

years from "benevolent" critics—and I am told the same thing even today—that psycho-analysis is right up to such-and-such a point but that there it begins to exaggerate and to generalize without justification. But I know that, while nothing is more difficult than to draw such a line, the critics had been completely ignorant of the whole subject only a few weeks or days earlier.

The result of the official anathema against psycho-analysis was that the analysts began to come closer together. At the second Congress, held at Nuremberg in 1910, they formed themselves, on the proposal of Ferenczi, into an "International Psycho-Analytical Association," divided into a number of local societies but under a common president. The Association survived the Great War and still exists, consisting today of branch societies in Austria, Germany, Hungary, Switzerland, Great Britain, Holland, Russia and India, as well as two in the United States. I arranged that C. G. Jung should be ap-

pointed as the first president, which turned out later to have been a most unfortunate step. At the same time a second journal devoted to psycho-analysis was started, the *Zentralblatt für Psycho-analyse,* edited by Adler and Stekel, and a little later a third, *Imago,* edited by two non-medical analysts, H. Sachs and O. Rank, and intended to deal with the application of analysis to the mental sciences. Soon afterwards Bleuler published a paper in defence of psycho-analysis.[1] Though it was a relief to find honesty and straightforward logic for once taking part in the dispute, yet I could not feel completely satisfied by Bleuler's essay. He strove too eagerly after an appearance of impartiality; nor is it a matter of chance that it is to him that our science owes the valuable concept of *ambivalence.* In later papers Bleuler adopted such a critical attitude towards the theoretical structure of analysis, and

[1] "Die Psychoanalyse Freuds," *Jahrbuch für psycho-analytische und psychopathologische Forschungen,* Bd. II, 1910.

rejected or threw doubts upon such essential parts of it, that I could not help asking myself in astonishment what could be left of it for him to admire. Yet not only has he subsequently uttered the strongest pleas in favor of "depth-psychology" but he based his comprehensive study of schizophrenia upon it. Nevertheless Bleuler did not for long remain a member of the International Psycho-Analytical Association; he resigned from it as a result of misunderstandings with Jung, and the Burghölzli [2] was lost to analysis.

Official disapproval could not hinder the spread of psycho-analysis either in Germany or in other countries. I have elsewhere [3] followed the stages of its growth and given the names of those who were its first representatives. In 1909 G. Stanley Hall invited Jung and me to America to go to the Clark University, Worcester, Mass., of which he was president, and to spend a week

[2] [The public mental hospital at Zurich.]
[3] "On the History of the Psycho-Analytic Movement."

giving lectures (in German) at the celebration of the twentieth anniversary of that body's foundation. Hall was justly esteemed as a psychologist and educationalist, and had introduced psychoanalysis into his courses some years before; there was a touch of the "king-maker" about him, a pleasure in setting up authorities and in then deposing them. We also met James J. Putnam there, the Harvard neurologist, who in spite of his age was an enthusiastic supporter of psychoanalysis and threw the whole weight of a personality that was universally respected into the defence of the cultural value of analysis and the purity of its aims. He was an estimable man, in whom, as a reaction against a predisposition to obsessional neurosis, an ethical bias predominated; and the only thing in him that we could regret was his inclination to attach psycho-analysis to a particular philosophical system and to make it the servant of moral aims. Another event of this time which made a lasting impression upon me was a meeting with William James the

philosopher. I shall never forget one little scene that occurred as we were on a walk together. He stopped suddenly, handed me a bag he was carrying and asked me to walk on, saying that he would catch me up as soon as he had got through an attack of angina pectoris which was just coming on. He died of that disease a year later; and I have always wished that I might be as fearless as he was in the face of approaching death.

At that time I was only fifty-three, I felt young and healthy, and my short visit to the new world encouraged my self-respect in every way. In Europe I felt as though I were despised, but in America I found myself received by the foremost men as an equal. As I stepped on to the platform at Worcester to deliver my *Five Lectures upon Psycho-Analysis* it seemed like the realization of some incredible day-dream: psycho-analysis was no longer a product of delusion; it had become a valuable part of reality. It has not lost ground in America since our visit; it is extremely popular among the lay public and is

recognized by a number of official psychiatrists as an important element in medical training. Unfortunately, however, it has suffered a great deal from being watered down. Moreover, many abuses which have no relation to it find a cover under its name, and there are few opportunities for any thorough training in technique or theory. In America, too, it has come in conflict with Behaviorism, a theory which is naïve enough to boast that it has put the whole problem of psychology completely out of court.

In Europe during the years 1911-1913 two secessionist movements from psycho-analysis took place, led by men who had previously played a considerable part in the young science, Alfred Adler and C. G. Jung. Both movements seemed most threatening and quickly obtained a large following. But their strength lay, not in their own content, but in the temptation which they offered of being freed from what were felt as the repellent findings of psycho-analysis without the necessity of rejecting its actual material. Jung

attempted to give to the facts of analysis a fresh interpretation of an abstract, impersonal and non-historical character, and thus hoped to escape the need for recognizing the importance of infantile sexuality and of the Oedipus complex, as well as the necessity for any analysis of childhood. Adler seemed to depart still further from psycho-analysis; he entirely repudiated the importance of sexuality, traced back the formation both of character and of the neuroses solely to men's desire for power and to their need to compensate for their constitutional inferiority, and threw all the psychological discoveries of psycho-analysis to the winds. But what he had rejected forced its way back into his closed system under other names; his "masculine protest" is nothing else than repression unjustifiably sexualized. The criticism with which the two heretics were met was a mild one; I only insisted that both Adler and Jung should cease to describe their theories as "psycho-analysis." After a lapse of ten years it can be asserted that both

of these attempts against psycho-analysis have blown over without doing any harm.

If a community is based upon agreement on a few cardinal points, it is obvious that people who have abandoned that common ground will cease to belong to it. Yet the secession of former pupils has often been brought up against me as a sign of my intolerance or has been regarded as evidence of some special fatality that hangs over me. It is a sufficient answer to point out that in contrast to those who have left me, like Jung, Adler, Stekel, and a few besides, there are a great number of men, like Abraham, Eitingon, Ferenczi, Rank, Jones, Brill, Sachs, Pfister, van Emden, Reik, and others, who have worked with me for some fifteen years in loyal collaboration and for the most part in uninterrupted friendship. I have only mentioned the oldest of my pupils, who have already made a distinguished name for themselves in the literature of psycho-analysis; if I have passed over others that is not to be taken as a slight, and indeed among those

who are young and have joined me lately talents are to be found on which great hopes may be set. But I think I can say in my defence that an intolerant man, dominated by an arrogant belief in his own infallibility, would never have been able to maintain his hold upon so large a number of intelligent people, especially if he had at his command as few practical attractions as I had.

The Great War, which broke up so many other organizations, could do nothing against our "International." The first meeting after the War took place in 1920, at The Hague, on neutral ground. It was moving to see how hospitably the Dutch welcomed the starving and impoverished subjects of the Central European states; and I believe this was the first occasion in a ruined world on which Englishmen and Germans sat at the same table for the friendly discussion of scientific interests. Both in Germany and in the countries of Western Europe the War had actually stimulated interest

in psycho-analysis. The observation of war neuroses had at last opened the eyes of the medical profession to the importance of psychogenesis in neurotic disturbances, and some of our psychological conceptions, such as the "advantage of being ill" and the "flight into illness," suddenly became popular. The last Congress before the German collapse, which was held at Budapest in 1918, was attended by official representatives of the allied governments of the Central European powers, and they agreed to the establishment of psycho-analytical stations for the treatment of war neuroses. But this point was never reached. Similarly, too, the comprehensive plans made by one of our leading members, Dr. Anton von Freund, for establishing in Budapest a center for analytic study and treatment, came to grief as a result of the political disorders of the time and of the premature death of their generous author. At a later date some of his ideas were put into execution by Max Eitingon, who in 1920 founded a psycho-

analytical clinic in Berlin. During the brief period of Bolshevist rule in Hungary, Ferenczi was able to carry on a successful course of instruction as the official representative of psychoanalysis at the University of Budapest. After the War our opponents announced with great joy that events had produced a conclusive argument against the validity of the theses of analysis. The war neuroses, they said, had proved that sexual factors were unnecessary to the aetiology of neurotic disorders. But their triumph was frivolous and premature. For on the one hand no one had been able to carry out a thorough analysis of a case of war neurosis, so that in fact nothing whatever was known for certain as to their motivation, and no conclusions could be drawn from this uncertainty; while on the other hand psycho-analysis had long before arrived at the concept of narcissism and of narcissistic neuroses, in which the subject's libido is attached to his own ego instead of to an object. Though on other occasions, therefore, the charge was

brought against psycho-analysis of having made an unjustifiable extension of the concept of sexuality, yet, when it became convenient for polemical ends, this crime was forgotten and we were once more held down to the narrowest meaning of the word.

If the preliminary cathartic period is left on one side, the history of psycho-analysis from my point of view falls into two phases. In the first of these I stood alone and had to do all the work myself: this was from 1895-96 until 1906 or 1907. In the second phase, lasting from then until the present time, the contributions of my pupils and collaborators have been growing more and more in importance, so that today, when a grave illness warns me of the approaching end, I can think with a quiet mind of the cessation of my own labors. For that very reason, however, it is impossible for me in this *Autobiographical Study* to deal as fully with the progress of psycho-analysis during the second phase as I did with its gradual rise during the first phase,

which was concerned with my own activity alone. I feel that I should only be justified in mentioning here those new discoveries in which I still played a prominent part, in particular, therefore, those made in the sphere of narcissism, of the theory of the instincts, and of the application of psycho-analysis to the psychoses.

I must begin by adding that increasing experience showed more and more plainly that the Oedipus complex was the nucleus of the neurosis. It was at once the climax of infantile sexual life and the point of junction from which all of its later developments proceeded. But if so, it was no longer possible to expect analysis to discover a factor that was specific in the aetiology of the neuroses. It must be true, as Jung expressed it so well in the early days when he was still an analyst, that neuroses have no peculiar content which belongs exclusively to them, but that neurotics break down at the same difficulties that are successfully overcome by normal people. This discovery was very far from

being a disappointment. It was in complete harmony with another one: that the depth-psychology revealed by psycho-analysis was in fact the psychology of the normal mind. Our path had been like that of chemistry: the great qualitative differences between substances were traced back to quantitative variations in the proportions in which the same elements were combined.

In the Oedipus complex the libido was attached to the image of the parents. But earlier there was a period in which there were no such objects. There followed from this fact the concept (of fundamental importance for the libido theory) of a state in which the subject's libido filled his own ego and had that for its object. This state could be called *narcissism* or self-love. A moment's reflection showed that this state never completely ceases. All through the subject's life his ego remains the great reservoir of his libido, from which the attachments to objects (the *object-cathexes*) radiate out and into

which the libido can stream back again from the objects. Thus narcissistic libido is constantly being converted into object-libido, and vice versa. An excellent instance of the length to which this conversion can go is afforded by the sexual or sublimated devotion which involves a sacrifice of the self. Whereas hitherto, in considering the process of repression, attention had only been paid to what was repressed, these ideas made it also possible to form a correct estimate of the repressing forces. It had been said that repression was set in action by the instincts of self-preservation operating in the ego (the *ego-instincts*) and that it was brought to bear upon the libidinal instincts. But since the instincts of self-preservation were now recognized as also being of a libidinal nature, as being narcissistic libido, the process of repression was seen to be a process occurring within the libido itself; narcissistic libido was opposed to object-libido; the interests of self-preservation defended themselves against the demands of object-love, that

is, against the demands of sexuality in the narrower sense.

There is no more urgent need in psychology than for a securely-founded theory of the instincts on which it might then be possible to build further. Nothing of the sort exists, however, and psycho-analysis is driven to making tentative efforts towards some such theory. It began by drawing a contrast between the ego-instincts (the instinct of self-preservation, hunger) and the libidinal instincts (love), but later replaced it by a new contrast between narcissistic and object-libido. This was clearly not the last word on the subject; biological considerations seemed to make it impossible to remain content with assuming the existence of only a single class of instincts.

In the works of my later years (*Beyond the Pleasure Principle, Group Psychology and the Analysis of the Ego,* and *The Ego and the Id*) [4]

[4] [*Jenseits des Lustprinzips,* Vienna, 1920, *Massenpsychologie und Ichanalyse,* Vienna, 1921, and *Das Ich und das Es,* Vienna, 1923.]

I have given free rein to the inclination which I kept down for so long to speculation and I have also taken stock of a new solution of the problem of the instincts. I have combined the instincts for self-preservation and for the preservation of the species under the concept of *Eros* and have contrasted with it an instinct of death or destruction which works in silence. Instinct in general is regarded as a kind of elasticity of living things, an impulsion towards the restoration of a situation which once existed but was brought to an end by some external disturbance. This essentially conservative character of instincts is exemplified by the phenomena of the *compulsion to repeat.* The picture which life presents to us is the result of the working of Eros and the death-instinct together and against each other.

It remains to be seen whether this construction will turn out to be serviceable. Although it arose from a desire to fix some of the most important theoretical ideas of psycho-analysis, it

goes far beyond psycho-analysis. I have repeatedly heard it said contemptuously that it is impossible to take a science seriously whose most general concepts are as lacking in precision as those of libido and of instinct in psycho-analysis. But this reproach is based upon a complete misconception of the facts. Clear fundamental concepts and sharply-drawn definitions are only possible in the mental sciences in so far as the latter seek to fit a department of facts into the frame of a logical system. In the natural sciences, of which psychology is one, such clear-cut general concepts are superfluous and indeed impossible. Zoölogy and botany did not start from correct and adequate definitions of an animal and a plant; to this very day biology has been unable to give any certain meaning to the concept of life. Physics itself, indeed, would never have made any advance if it had had to wait until its concepts of matter, force, gravitation, and so on, had reached the desirable degree of clarity and precision. The fundamental concepts or

most general ideas in any of the disciplines of science are always left indeterminate at first and are only explained to begin with by reference to the realm of phenomena from which they were derived; it is only by means of a progressive analysis of the material of observation that they can be made clear and can find a significant and consistent meaning. I have always felt it as a gross injustice that people always refused to treat psycho-analysis like any other science. This refusal found an expression in the raising of the most obstinate objections. Psycho-analysis was constantly reproached for its incompletenesses and insufficiencies, though it is plain that a science based upon observation has no alternative but to work out its findings piecemeal and to solve its problems step by step. Again, when I endeavored to obtain for the sexual function the recognition which had so long been withheld from it, psycho-analytic theory was branded as "pan-sexualism." And when I laid stress upon the hitherto neglected importance of the part

played by the accidental impressions of early youth, I was told that psycho-analysis was denying constitutional and hereditary factors—a thing which I had never dreamed of doing. It was a case of contradiction at any price and by any methods.

I had already made attempts at earlier stages of my work to arrive at some more general points of view, starting from the observations of psycho-analysis. In a short essay, "Formulations regarding the Two Principles of Mental Functioning," [5] published in 1911, I drew attention (and there was, of course, nothing original in this) to the domination of the *pleasure-pain principle* in mental life and to its displacement by the so-called *reality principle*. Later on (1915-17), I made an attempt to produce a "Metapsychology." By this I meant a method of approach according to which every mental process is considered in relation to three co-ordinates, which

[5] [*Jahrbuch für psychoanalytische und psychopathologische Forschungen*, Bd. III, 1911. English translation in Freud's *Collected Papers*, Vol IV.]

I described as *dynamic, topographical,* and *economic* respectively; and this seemed to me to represent the furthest goal that psychology could attain. The attempt remained no more than a torso; after writing two or three papers—"Instincts and their Vicissitudes," "Repression," "The Unconscious," "Mourning and Melancholia," etc.[6]—I broke off, wisely perhaps, since the time for theoretical predications of this kind had not yet come. In my latest speculative works I have set about the task of dissecting our mental apparatus on the basis of the analytic view of pathological facts and have divided it into an *ego,* an *id,* and a *super-ego.*[7] The super-ego is the heir of the Oedipus complex and represents the ethical standards of mankind.

I should not like to create an impression that during this last period of my work I have turned my back upon patient observation and have

[6] [Published in the *Zeitschrift für Psychoanalyse* between 1915 and 1917. English translation in Freud's *Collected Papers,* Vol. IV.]

[7] *The Ego and the Id.*

abandoned myself entirely to speculation. I have on the contrary always remained in the closest touch with the analytic material and have never ceased working at detailed points of clinical or technical importance. Even when I have moved away from observation, I have carefully avoided any contact with philosophy proper. This avoidance has been greatly facilitated by constitutional incapacity. I was always open to the ideas of G. T. Fechner and have followed that thinker upon many important points. The large extent to which psycho-analysis coincides with the philosophy of Schopenhauer—not only did he assert the dominance of the emotions and the supreme importance of sexuality but he was even aware of the mechanism of repression—is not to be traced to my acquaintance with his teaching. I read Schopenhauer very late in my life. Nietzsche, another philosopher whose guesses and intuitions often agree in the most astonishing way with the laborious findings of psycho-analysis, was for a long time avoided by me on that very

account; I was less concerned with the question of priority than with keeping my mind unembarrassed.

The neuroses were the first subject of analysis and for a long time they were the only one. No analyst could doubt that medical practice was wrong in separating those disorders from the psychoses and in attaching them to the organic nervous diseases. The theory of the neuroses belongs to psychiatry and is indispensable as an introduction to it. It would seem, however, that the analytical study of the psychoses is impracticable, owing to its lack of therapeutic results. Mental patients are as a rule without the capacity for forming a positive transference, so that the principal instrument of analytic technique is inapplicable to them. There are nevertheless a number of methods of approach to be found. Transference is often not so completely absent but that it can be used to a certain extent; and analysis has achieved undoubted successes with cyclical depressions, light paranoic modifica-

tions and partial schizophrenias. It has at least been a benefit to science that in many cases the diagnosis can oscillate for quite a long time between assuming the presence of a psychoneurosis or of a dementia praecox; for therapeutic attempts initiated in such cases have resulted in valuable discoveries before they have had to be broken off. But the chief consideration in this connection is that so many things that in the neuroses have to be laboriously fetched up from the depths are found in the psychoses upon the surface, visible to every eye. So that the best subjects for the demonstration of many of the assertions of analysis are provided by the psychiatric clinic. It was thus bound to happen before long that analysis would find its way to the objects of psychiatric observation. At a very early date (1896) I was able to establish in a case of paranoid dementia the presence of the same aetiological factors and the same emotional complexes as in the neuroses. Jung explained some most puzzling stereotypies in de-

ments by bringing them into relation with the patients' life histories; Bleuler demonstrated the existence in various psychoses of mechanisms like those which analysis had discovered in neurotics. Since then analysts have never relaxed their efforts to come to an understanding of the psychoses. Especially since it has been possible to work with the concept of narcissism, they have managed, now in this place and now in that, to get a glimpse beyond the wall. Most of all, no doubt, was achieved by Abraham in his elucidation of melancholia. It is true that in this sphere all our knowledge is not yet converted into therapeutic power; but the mere theoretical gain is not to be despised, and we may be content to wait for its practical application. In the long run even the psychiatrists have been unable to resist the convincing force of their own clinical material. At the present time German psychiatry is undergoing a kind of "peaceful penetration" by analytic views. While they continually declare that they will

never be psycho-analysts, that they do not belong to the "orthodox" school or agree with its exaggerations, and in particular that they do not believe in the predominance of the sexual factor, nevertheless the majority of the younger workers take over one piece or another of analytical theory and apply it in their own fashion to the material. All the signs point to the proximity of further developments in the same direction.

VI

I NOW watch from a distance the symptomatic reactions that are accompanying the introduction of psycho-analysis into the France which was for so long refractory. It seems like a reproduction of something I have lived through before, and yet it has peculiarities of its own. Objections of incredible simplicity are raised, such as that French sensitiveness is offended by the pedantry and crudity of psycho-analytical terminology. (One cannot help being reminded of Lessing's immortal Chevalier Riccaut de la Marlinière.[1]) Another comment has a more serious ring (a professor of psychology at the

[1] [The comic French soldier of fortune in *Minna von Barnhelm* who is amazed when his sharp practice at cards is described as cheating: "Comment Mademoiselle? Vous appelez cela 'cheating'? Corriger la fortune, l'enchaîner sous ses doigts, être sûr de son fait—do the Germans call that 'cheating'? Cheating! Oh, what a poor language, what a crude language German must be!"]

Sorbonne did not think it beneath him) : the whole method of thought of psycho-analysis is inconsistent with the *génie latin.* Here the Anglo-Saxon allies of France, who count as supporters of analysis, are explicitly thrown over. Anyone hearing the remark would suppose that psycho-analysis had been the favorite child of the *génie teutonique* and had been clasped to its heart from the moment of birth.

In France the interest in psycho-analysis began among the men of letters. To understand this, it must be borne in mind that from the time of the writing of *The Interpretation of Dreams* psycho-analysis ceased to be a purely medical subject. Between its appearance in Germany and in France lies the history of its numerous applications to departments of literature and of aesthetics, to the history of religions and to pre-history, to mythology, to folk-lore, to education, and so on. None of these things have much to do with medicine; in fact it is only through psycho-analysis that they are con-

nected with it. I have no business, therefore, to go into them in detail in these pages.[2] I cannot pass them over completely in silence, however, for on the one hand they are essential to a correct appreciation of the nature and value of psycho-analysis, and on the other hand I have, after all, undertaken to give an account of my life-work. The beginnings of the majority of these applications of psycho-analysis will be found in my works. Here and there I have gone a little way along the path in order to gratify my non-medical interests. Later on, others (not only doctors, but specialists in the various fields as well) have followed in my tracks and penetrated far into the different subjects. But since my program limits me to a mention of my own share in these applications of psycho-analysis, I can only give a quite inadequate picture of their extent and importance.

A number of suggestions came to me out of

[2] [The present work, it will be remembered, originally formed part of a series of medical autobiographies.]

the Oedipus complex, the ubiquity of which gradually dawned on me. The poet's choice, or his invention, of such a terrible subject seemed puzzling; and so, too, did the overwhelming effect of its dramatic treatment, and the general nature of such tragedies of destiny. But all of this became intelligible when one realized that a universal law of mental life had here been captured in all its emotional significance. Fate and the oracle were no more than materializations of an internal necessity; and the fact of the hero sinning without his knowledge and against his intentions was evidently a right expression of the *unconscious* nature of his criminal tendencies. From understanding this tragedy of destiny it was only a step further to understanding a tragedy of character—*Hamlet,* which had been admired for three hundred years without its meaning being discovered or its author's motives guessed. It could scarcely be chance that this neurotic creation of the poet should have broken down, like his numberless

fellows in the real world, at the Oedipus complex; for Hamlet was faced with the task of taking vengeance upon another for the two deeds which are the subject of the Oedipus desires, and before that task his arm was paralyzed by his own obscure sense of guilt. Shakespeare wrote *Hamlet* very soon after his father's death.[3] The suggestions made by me for the analysis of this tragedy were fully worked out later on by Ernest Jones. And the same example was afterwards used by Otto Rank as the starting-point for his investigation of the choice of material made by dramatists. In his large volume upon the incest theme [4] he was able to show how often imaginative writers have taken as their subject the themes of the Oedipus situation, and traced

[3] (Additional Note, 1935:) This is a construction which I should like expressly to withdraw. I no longer believe that William Shakespeare, the actor from Stratford, was the author of the works that have been ascribed to him. Since reading *Shakespeare Identified,* by J. T. Looney, I am almost convinced that the assumed name conceals the personality of Edward De Vere, Earl of Oxford.

[4] *Das Inzest-Motiv in Dichtung und Sage,* Vienna, 1912.

in the different literatures of the world the way in which the material has been transformed, modified and softened.

It was tempting to go on from there to an attempt at an analysis of poetic and artistic creation in general. The realm of imagination was evidently a "sanctuary" made during the painful transition from the pleasure principle to the reality principle in order to provide a substitute for the gratification of instincts which had to be given up in real life. The artist, like the neurotic, had withdrawn from an unsatisfying reality into this world of imagination; but, unlike the neurotic, he knew how to find a way back from it and once more to get a firm foothold in reality. His creations, works of art, were the imaginary gratifications of unconscious wishes, just as dreams are; and like them they were in the nature of compromises, since they, too, were forced to avoid any open conflict with the forces of repression. But they differed from the asocial, narcissistic products of dreaming in

that they were calculated to arouse interest in other people and were able to evoke and to gratify the same unconscious wishes in them, too. Besides this, they made use of the perceptual pleasure of formal beauty as what I have called an "incitement-premium." What psycho-analysis was able to do was to take the inter-relations between the impressions of the artist's life, his chance experiences and his works, and from them to construct his constitution and the impulses at work in it—that is to say, that part of him which he shared with all men. With this aim in view, for instance, I made Leonardo da Vinci the subject of a study which is based upon a single memory of childhood related by him, and which aims chiefly at explaining his picture of "St. Anne with the Virgin and Child." It does not appear that the enjoyment of a work of art is spoiled by the knowledge gained from such an analysis. The layman may perhaps expect too much from analysis in this respect, for it must be admitted that it throws no light upon the

two problems which probably interest him the most. It can do nothing towards elucidating the nature of the artistic gift, nor can it explain the means by which the artist works—artistic technique.

I was able to show from a short story by W. Jensen called *Gradiva,* which has no particular merit in itself, that invented dreams can be interpreted in the same way as real ones and that the unconscious mechanisms familiar to us in the "dream-work" are thus also operative in the processes of imaginative writing. My book upon *Wit and Its Relation to the Unconscious* [5] was a side-issue indirectly derived from *The Interpretation of Dreams.* The only friend of mine who was at that time interested in my work remarked to me that my interpretations of dreams often impressed him as being like jokes. In order to throw some light on this impression I began to investigate jokes, and found that their essence

[5] [First German edition, under the title of *Der Witz und seine Beziehung zum Unbewussten,* Vienna, 1905.]

lay in the technical methods employed in them, and that these were the same as the means used in the "dream-work"—that is to say, condensation, displacement, the representation of a thing by its opposite or by a triviality, and so on. This led to an economic inquiry as to the origin of the high degree of pleasure obtained from hearing a joke. And to this the answer was that it was due to the momentary suspension of the energy expended upon maintaining repression, owing to the attraction exercised by the offer of a premium of pleasure (*fore-pleasure*).

I myself set a higher value upon my contributions to the psychology of religion, which began in 1907 with the establishment of a remarkable similarity between obsessive acts and religious practices or ritual. Without as yet understanding the deeper connections, I described the obsessional neurosis as a distorted private religion and religion as a kind of universal obsessional neurosis. Later on, in 1912, the explicit indications of Jung as to the far-reaching analogies be-

tween the mental products of neurotics and of primitive peoples led me to turn my attention to that subject. In four essays, which were collected into a book with the title of *Totem and Taboo,*[6] I showed that the dread of incest was even more marked among primitive than among civilized races and had given rise to very special measures of defence against it; I examined the relations between taboo-prohibitions (the earliest form in which moral restrictions make their appearance) and emotional ambivalence; and I discovered under the primitive scheme of the universe known as animism the principle of the overestimation of the importance of psychical reality, the principle of "the omnipotence of thoughts," which also lies at the root of magic. I developed the comparison with the obsessional neurosis at every point, and showed how many of the postulates of primitive mental life are still in force in that remarkable disorder. Above

[6] [First German edition, under the title of *Totem und Tabu,* Vienna, 1913.]

all, however, I was attracted by totemism, the first system of organization in primitive tribes, a system in which the beginnings of social order are united with a rudimentary religion and the implacable domination of a small number of taboo-prohibitions. The being that is honored is ultimately always an animal, from which the clan also claims to be descended. Many indications pointed to the conclusion that every race, even the most highly developed, had once passed through the stage of totemism.

The chief literary sources of my studies in this field were the well-known works of J. G. Frazer (*Totemism and Exogamy* and *The Golden Bough*), a mine of valuable facts and opinions. But Frazer effected little towards elucidating the problems of totemism; he had several times fundamentally altered his views on the subject, and the other ethnologists and pre-historians seemed in equal uncertainty and disagreement. My starting-point was the striking correspondence between the two taboo-in-

junctions of totemism (not to kill the totem and not to have sexual relations with any woman of the same totem-clan) and the two elements of the Oedipus complex (killing the father and taking the mother to wife). I was therefore tempted to equate the totem-animal with the father; and in fact primitive peoples themselves do this explicitly, by honoring it as the forefather of the clan. There next came to my help two facts from psycho-analysis, a lucky observation of a child made by Ferenczi, which made it possible to speak of an "infantile return of totemism," and the analysis of early animal-phobias in children, which so often showed that the animal was a substitute for the father, a substitute on to which the fear of the father derived from the Oedipus complex had been displaced. Not much was lacking to enable me to recognize the killing of the father as the nucleus of totemism and the starting-point in the formation of religion.

This missing element was supplied when I

became acquainted with W. Robertson Smith's work, *The Religion of the Semites.* Its author (a man of genius who was both a physicist and a biblical expert) introduced the so-called totem-feast as an essential part of the totemistic religion. Once a year the totem animal, which was at other times regarded as sacred, was solemnly killed in the presence of all the members of the clan, was devoured and was then mourned over. The mourning was followed by a great festival. When I further took into account Darwin's conjecture that men originally lived in hordes, each under the domination of a single powerful, violent and jealous male, there rose before me out of all these components the following hypothesis, or, I would rather say, vision. The father of the primal horde, since he was an unlimited despot, had seized all the women for himself; his sons, being dangerous to him as rivals, had been killed or driven away. One day, however, the sons came together and united to overwhelm, kill, and devour their father, who had been their enemy but

also their ideal. After the deed they were unable to take over their heritage since they stood in one another's way. Under the influence of failure and regret they learned to come to an agreement among themselves; they banded themselves into a clan of brothers by the help of the ordinances of totemism, which aimed at preventing a repetition of such a deed, and they jointly undertook to forego the possession of the women on whose account they had killed their father. They were then driven to finding strange women, and this was the origin of the exogamy which is so closely bound up with totemism. The totem-feast was the commemoration of the fearful deed, from which sprang man's sense of guilt (or "original sin") and which was the beginning at once of social organization, of religion and of ethical restrictions.

Now whether we suppose that such a possibility was a historical event or not, it brings the formation of religion within the circle of the father-complex and bases it upon the am-

bivalence which dominates that complex. After the totem animal had ceased to serve as a substitute for him, the primal father, at once feared and hated, honored and envied, became the prototype of God himself. The son's rebelliousness and his affection for his father struggled against each other through a constant succession of compromises, which sought on the one hand to atone for the act of parricide and on the other to consolidate the advantages it had brought. This view of religion throws a particularly clear light upon the psychological basis of Christianity, in which, it may be added, the ceremony of the totem-feast still survives, with but little distortion, in the form of Communion. I should like explicitly to mention that this last observation was not made by me but is to be found in the works of Robertson Smith and Frazer.

Theodor Reik and G. Róheim, the ethnologist, have taken up the line of thought which I developed in *Totem and Taboo* and, in a series

of important works, have extended it, amplified it or corrected it. I myself have since returned to it more than once, in the course of my investigations into the "unconscious sense of guilt" (which also plays such an important part among the motives of neurotic suffering) and in my attempts at forming a closer connection between social psychology and the psychology of the individual.[7] I have, moreover, made use of the idea of an archaic inheritance from the "primal horde" epoch of mankind's development in explaining susceptibility to hypnosis.

I have taken but little direct part in certain other applications of psycho-analysis, though they are none the less of general interest. It is only a step from the phantasies of individual neurotics to the imaginative creations of groups and peoples as we find them in myths, legends and fairy tales. Mythology became the special province of Otto Rank; the interpretation of

[7] *The Ego and the Id* and *Group Psychology and the Analysis of the Ego.*

myths, the tracing of them back to the familiar unconscious complexes of infancy, the replacing of astral explanations by a discovery of human motives, all of this is to a large extent due to his analytic efforts. The subject of symbolism has also found many students among my followers. Symbolism has brought psycho-analysis many enemies; many inquirers with unduly prosaic minds have never been able to forgive it the recognition of symbolism, which followed from the interpretation of dreams. But analysis is guiltless of the discovery of symbolism, for it had long been known in other regions of thought (such as folk-lore, legends and myths) and plays an even larger part in them than in the "language of dreams."

I myself have contributed nothing to the application of analysis to education. It was natural, however, that the analytic discoveries as to the sexual life and mental development of children should attract the attention of educators and make them see their problems in a new light.

Dr. Oskar Pfister, a Protestant pastor at Zurich, led the way as a tireless pioneer along these lines, nor did he find the practice of analysis incompatible with the retention of his religion, though it is true that this was of a sublimated kind. Among the many others who worked alongside him I may mention Frau Dr. Hug-Hellmuth and Dr. S. Bernfeld, both of Vienna.[8] The application of analysis to the prophylactic education of healthy children and to the correcting of those who, though not actually neurotic, have deviated from the normal course of development, has led to one consequence which is of practical importance. It is no longer possible to restrict the practice of psycho-analysis to physicians and to exclude laymen from it. In fact, a physician who has not been through a special training is, in spite of his diploma, a layman in analysis, and a non-physician who has been suitably trained

[8] (Additional Note, 1935:) Since these words were written child analysis in particular has gained a powerful momentum owing to the work of Mrs. Melanie Klein and of my daughter, Anna Freud.

can, with occasional reference to a physician, carry out the analytic treatment not only of children but also of neurotics.

By a process of development against which it would have been useless to struggle, the word "psycho-analysis" has itself become ambiguous. While it was originally the name of a particular therapeutic method, it has now also become the name of a science—the science of unconscious mental processes. By itself this science is seldom able to deal with a problem completely, but it seems destined to give valuable contributory help in a large number of regions of knowledge. The sphere of application of psycho-analysis extends as far as that of psychology, to which it forms a complement of the greatest moment.

Looking back, then, over the patchwork of my life's labors, I can say that I have made many beginnings and thrown out many suggestions. Something will come of them in the future, though I cannot myself tell whether it will be

much or little. I can, however, express a hope that I have opened up a pathway for an important advance in our knowledge.

POSTCRIPT (1935)

THE editor of this series of autobiographical studies did not, so far as I know, consider the possibility that after a certain lapse of time a sequel might be written to any of them; and it may be that such an event has occurred only in the present instance. I am undertaking the task since my American publisher desires to issue the little work in a new edition. It first appeared in America in 1927 (published by Brentano) under the title of *An Autobiographical Study,* but it was injudiciously brought out in the same volume as another essay of mine which gave its title, *The Problem of Lay-Analyses,* to the whole book and so obscured the present work.

Two themes run through these pages: the story of my life and the history of psychoanalysis. They are intimately interwoven. This

Autobiographical Study shows how psychoanalysis came to be the whole content of my life and rightly assumes that no personal experiences of mine are of any interest in comparison to my relations with that science.

Shortly before I wrote this study it seemed as though my life would soon be brought to an end by the recurrence of a malignant disease; but surgical skill saved me in 1923 and I was able to continue my life and my work, though no longer in freedom from pain. In the period of more than ten years that has passed since then, I have never ceased my analytic work nor my writing—as is proved by the completion of the twelfth volume of the German edition of my collected works. But I myself find that a significant change has come about. Threads which in the course of my development had become intertangled have now begun to separate; interests which I had acquired in the later part of my life have receded, while the older and original ones become prominent once more. It is true that

in this last decade I have carried out some important pieces of analytic work, such as the revision of the problem of anxiety in my book, *Hemmung, Symptom und Angst* (published in 1926), or the simple explanation of sexual "fetishism" which I was able to make in 1927. Nevertheless it would be true to say that, since I put forward my hypothesis of the existence of two kinds of instinct (Eros and the death instinct) and since I proposed a division of the mental personality into an ego, a super-ego and an id (in 1923), I have made no further decisive contributions to psycho-analysis; what I have written on the subject since then has been either unessential or would soon have been supplied by someone else. This circumstance is connected with an alteration in myself, with what might be described as a phase of regressive development. My interest, after making a life-long detour through the natural sciences, medicine and psycho-therapy, returned to the cultural problems which had fascinated me long before, when

I was a youth scarcely old enough for thinking. At the very climax of my psycho-analytic work, in 1912, I had already attempted in *Totem and Taboo* to make use of the newly-discovered findings of analysis in order to investigate the origins of religion and morality. I now carried this work a stage further in two later essays, *The Future of an Illusion* (1927) and *Civilization and Its Discontents* (1930). I perceived ever more clearly that the events of human history, the interactions between human nature, cultural development and the precipitates of primeval experiences (the most prominent example of which is religion) are no more than a reflection of the dynamic conflicts between the ego, the id and the super-ego, which psycho-analysis studies in the individual—are the very same processes repeated upon a wider stage. In *The Future of an Illusion* I expressed an essentially negative valuation of religion. Later I found a formula which did better justice to it: while granting that its power lies in the truth which it contains, I

showed that that truth was not a material but an historical truth.

These studies, which, though they originate in psycho-analysis, stretch far beyond it, have perhaps awakened more public sympathy than psycho-analysis itself. They may have played a part in creating the short-lived illusion that I was among the writers to whom a great nation like Germany was ready to listen. It was in 1929 that, with words no less pregnant than friendly, Thomas Mann, one of the acknowledged spokesmen of the German people, found a place for me in the history of modern thought. A little later my daughter Anna, acting as my proxy, was given a civic reception in the Rathaus at Frankfort-on-Main on the occasion of my being awarded the Goethe Prize for 1930. This was the climax of my life as a citizen. Soon afterwards the boundaries of our country narrowed and the nation would know no more of us.

And here I may be allowed to break off these autobiographical notes. The public has no

claim to learn any more of my personal affairs—of my struggles, my disappointments and my successes. I have in any case been more open and frank in some of my writings (such as *The Interpretation of Dreams* and *The Psychopathology of Everyday Life*) than people usually are who describe their lives for their contemporaries or for posterity. I have had small thanks for it and from my experience I cannot recommend anyone to follow my example.

I must add a few more words upon the history of psycho-analysis during the last decade. There can no longer be any doubt that it will continue; it has proved its capacity to survive and to develop both as a branch of knowledge and as a therapeutic method. The number of its supporters (organized into the International Psycho-Analytical Association) has considerably increased. In addition to the older local groups (in Vienna, Berlin, Budapest, London, Holland, Switzerland and Russia), societies have since been formed in Paris and Calcutta, two in

Japan, several in the United States, and quite recently one each in Jerusalem and South Africa and two in Scandinavia. Out of their own funds these local societies support (or are in process of forming) training institutes, in which instruction in the practice of psycho-analysis is given according to a uniform plan, and out-patient clinics, in which experienced analysts as well as students give free treatment to patients of limited means. Every other year the members of the International Psycho-Analytical Association hold a Congress at which scientific papers are read and questions of organization decided. The thirteenth of these congresses (which I myself can no longer attend) took place at Lucerne in 1934. From a core of interests that are common to all members of the Association, their work radiates in many different directions. Some lay most stress upon clarifying and deepening our knowledge of psychology, while others are concerned with keeping in contact with medicine and psychiatry. From the practical point of view,

some analysts have set themselves the task of bringing about the recognition of psycho-analysis at the universities and its inclusion in the medical curriculum, whereas others are content to remain outside these institutions and maintain that psycho-analysis is no less important in the field of education than in that of medicine. It happens from time to time that an analytic worker may find himself isolated in an attempt to emphasize some single one of the findings or views of psycho-analysis at the expense of all the rest. Nevertheless, the whole impression is a satisfactory one—of serious scientific work carried on at a high level.

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